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ABSTRACT

This document consists of the three 1988 issues of the normally quarterly journal "Notes on Linguistics." These issues include the following significant articles: "The Role of the Field Linguist" (Bernard Comrie); "Relational Grammar: An Update Report" (Stephen A. Marlett); "Tone and Stress Analysis by Computer" (Geoffrey Hunt); "Correlations Between Vocabulary Similarity and Intelligibility" (Joseph E. Grimes); "A Sentence-Based Method for Intelligibility Testing" (Larry B. Jones); "Computing in Linguistics: Studying Morphophonemic Alternation in Annotated Text, Part One" (Gary F. Simons); "A Brief Note Regarding Discourse-Centered Ph.D. Studies at University of Texas at Arlington" (Robert E. Longacre); "CV-Analysis" (Ursula Wiesemann); "Roots and Concerns of Typological/Functional Linguistics" (Thomas E. Payne); "On the Renarrative Mood in Bulgarian" (Aleksander B. Milch); "Niger-Congo Noun Class Systems" (Ursula Wiesemann); "Correlations Between Vocabulary Similarity and Intelligibility" (Barbara F. Grimes); "How to Make Your Junior Partner an Equal Partner" (Francis L. Gralow); "Categorized and Prioritized Bibliography on Language Typology" (Thomas E. Payne); "Researching the Verb" (Ursula Wiesemann); "Comments on Analyzing Expository Discourse" (Sherri Brainard); "In Pursuit of Discourse Particles" (Linda K. Jones); and "The Function of the Glottal Stop in Gahuku" (Ellis W. Deibler, Jr.). Conference summaries, book reviews, and professional announcements are also included. (MSE)

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COORDINATOR'S CORNER

Eugene Loos

From time to time field workers have been privileged to have men like Givon, Comrie, and Lehmann give of their time and skills to stimulate and instruct during field workshops. We appreciate their contribution to our efforts to do our task. I am especially glad to have Bernard Comrie's article for this issue: I hope you find it both helpful and encouraging. It was to me.

Steve Marlett has for several years now had a unique on-going program that he calls 'maintenance'. Each member of one of his Relational Grammar courses at UND gets an annual update on what's new in Relational Grammar theory, with comments on new bibliography. He didn't keep the gem to himself: he made the suggestion some months back that we invite different people who are current on some linguistic topic, to provide an update article on a linguistic theory under development so that we could include one in each issue. I am glad to offer his article here as the first of the series. SIL members, do remember that the Academic Book Center (formerly the Bookstore) here at Dallas is willing to help you order books if you find it difficult because of currency or other limitations where you are.

At the Boston University African Language Conference in mid April, nearly all of the phonology papers presented were on tone. Some of the papers were challenged because of discrepancies about the accuracy of the data presented. Who of us hasn't felt the need of being able to demonstrate visually certain difficult segmental or suprasegmental features, even if only to convince ourselves of the accuracy of our detection? Geoff Hunt's new invention for tone and stress analysis is certain to be a much-wanted tool for field workers. Arrangements are under way for it to be produced. As you will note from his article, it requires only a lap-top computer. It would have been wonderful to have such an inexpensive little device at the Boston Conference.

Joe Grimes is plugging away at this book on how to approach semantics as the beginning point in field work. He writes that he is struggling with some central issues, but it is coming along. Almost at the other end of the spectrum, comparing the phonological similarity of lexical items, is his article included in this volume. Notice that even a little Tandy 100 is able to handle the short Basic program that Joe includes in his article, to make the comparison faster.

The purpose of making a comparison determines, of course, the nature of the data that one would want to compare, but we are fortunate to have for this same issue Jones' article on an 'effort to get beyond the word in determining language closeness'. I hope you will read it in connection with Grimes' article and start a dialogue that will encourage both authors.

To keep us from being ingrown, SIL graduate students have been encouraged to train under leading linguists outside the pale of SIL influence. We need the different insights and theoretical viewpoints. Yet we need to remember that Ken Pike has just gotten back from a month in the Soviet Union where he was requested to lecture at the academy of Science, and Bob Longacre has been requested to visit a number of universities abroad during the coming year. Others recognize them as having something significant to say, so Bob's article in this issue to remind us of opportunity close at hand is welcome.

A suggestion: as you scan the reports from professional meetings, ask yourself, 'What is currently of interest?' It might help you prepare for an upcoming meeting. ■

THE ROLE OF THE FIELD LINGUIST

Bernard Comrie
University of Southern California

One of the strengths of the Summer Institute of Linguistics within the general linguistic community is the wide range of languages on which its members are conducting fieldwork. One question that arises from this is: How best can SIL use this whole? What follow are some of my own reflections on this topic. I speak as a non-SIL member, though one who has on several occasions collaborated with SIL members, most recently at workshops in Papua New Guinea (1985) and Cameroon (1987).

In the present climate, there has developed an unfortunate schism between linguistic theoreticians and descriptive linguists, a schism that is unfortunate for both sides: Only too often, linguistic theory has turned a blind eye to the range of variety that is found among the languages of the world, concentrating its attention on the handful of languages that happen to be easy of access; while descriptive linguists, conscious of the arrogance that some theoretical linguists seem to display towards them, have turned in on their own tasks, neither taking from nor contributing to general linguistics. In my own work, I have tried to bridge this gap, since I am driven both by the desire to find out about Language and by the desire to find out about languages, two tasks that are more often in harmony than is often believed. I see signs elsewhere that the division between those who work on theory and those who work on languages is being broken down: formal grammarians no longer work just on English, while the voices of those who work on languages typologically far removed from English are being paid heed even by mainstream grammatical theory.

This does not mean, however, that the distinction between theory and fieldwork will simply disappear, since the number of those that make major contributions to both areas will probably always be small: I am thinking of fieldworker–theoreticians of the status of Kenneth Pike or R.M.W. Dixon, to cite one example from within SIL and one from outside. What is crucial is, rather, that theoreticians and fieldworkers should continue to develop respect for each other and for each other's work: there is no telling when a piece of information from the one side will prove crucial in resolving a debate within the other.

It follows from this that there is no need for the good linguistic fieldworker to try to emulate theoretical linguists, especially where this detracts from the quality of the fieldwork in return for at most a minimal contribution to theory. I realize that this path of abandoning fieldwork for theory is only too tempting, in a discipline which has seen the preeminence of theory and where the best academic positions have been seen to fall into the laps of theoreticians. But given the competition among theoreticians, even this practical concern of employability is scarcely relevant: it takes a committed and talented theoretician to survive the grueling competition for academic security, and for the talented fieldworker who is not by inclination a theoretician to enter this competition would only be to waste those talents that could be of real benefit to linguistics.

The conclusion towards which I am working is the following: The best way in which the fieldworker linguist can contribute to the development of linguistics is by carrying out good fieldwork, in other words by providing good descriptions of languages, or parts of languages, including both grammar and lexicon. Over the history of linguistics, theories have come and gone, and one would not expect this situation to change; the best descriptive grammars and dictionaries, however, remain as lasting testimonials to the efforts of their compilers.

One way in which the impact of theory on linguistic fieldwork has, in my opinion, had a negative effect is in encouraging detailed work on specific problems to the exclusion of gaining a general picture of how the language works as a whole. This is not to say that detailed studies of particular problems are not important: they clearly are, but only at the stage where one can see how they fit into the more general picture of the structure of a language. For instance, in consulting with SIL linguists carrying out fieldwork on the tense–aspect systems of languages of Cameroon, I often found myself at an impasse because I had no general idea of the structure of the language, for instance, of how the morphemes occurring in the expression of temporality function in the rest of the language. In some cases, the linguists I was working with had prepared sketch grammars of the languages in question, and in these cases it was possible to continue making progress. Thus, one of the early stages in fieldwork on a language should be the preparation of a sketch grammar, including the phoneme inventory, the major morphological categories and their expression, and the major syntactic constructions. Only subsequently should detailed studies of individual topics be undertaken. One should never forget that those who work on detailed problems of English grammar can take for granted the cumulative achievements of centuries of work on the overall structure of the English language; the fieldworker is unable to do this.

Another disadvantage of the preoccupation with theory is the neglect of collecting a broad corpus in favor of elicited material that addresses the particular theoretical point in question. At a later stage, when one already has the basics of the language described, elicitation is often the most efficient means of filling in gaps in the detailed description (e.g. of finding out whether one can relativize on objects of comparison, a construction that is likely to be extremely rare even if grammatically possible). Only by concentrating initially on a broad range of natural material, preferably not just restricted to narrative, can one be sure that the major constructions of the language have indeed been covered. In several instances where I have myself used elicitation as a shortcut, I have been forced to backtrack when textual material has shown that my original analysis was erroneous because I had not investigated the example in question against a more general background of the language. I would also emphasize the point made above, namely that textual material should not be restricted to narrative: Narrative is a rather specialized genre, in many cultures having its own particular structure, and the overall picture of a language can be distorted by concentrating on narrative to the exclusion of other genres (e.g. descriptions of processes, conversation, which latter is surely the most basic genre of human communication, even though unfortunately the most difficult to analyze).

Does this mean that the fieldworker can afford to neglect theory? By no means. Especially at the more advanced stages of work on a language, the results achieved by theoretical linguistics are likely to suggest solutions, or at least approaches, to problems that have been worrying the fieldworker. Essentially, this is just part of a more general injunction: Take advantage of what others have achieved before, rather than attempting to solve each problem anew on one's own. Because of the nature of the fieldwork undertaken by SIL members, fieldworkers are often in remote locations with little opportunity to communicate with outside linguists. For this reason, advantage should be taken of every opportunity to do so, in particular of workshops: Workshops provide not only the opportunity to consult with theoretical linguists from inside and outside SIL, but also to consult with one's colleagues, especially those who are at a more advanced level of study of genetically related or typologically similar languages. At workshops of this kind, it would be advisable (though I realize not always practical) to have a senior consultant for each of the language families or typological groups represented. At the workshop in Cameroon, both the participants and I benefited from the contributions of members with greater experience in working

on Bantu languages, and regretted the absence of a more senior consultant working on Chadic languages.

The pressures of life in the twentieth century are leading to increasing homogenization of humanity, with many cultures and languages in imminent danger of extinction. We owe it both to the members of these cultures or speakers of these languages and to posterity to record the contributions that these cultures and languages can make to our understanding of Man as a whole. In this sense, field linguists working with SIL are literally sitting on a gold mine. In many parts of the world, SIL members are the only, or the major, linguists working on indigenous languages, and are therefore the only regular sources of information on these languages. SIL can make an irreplaceable contribution to linguistics here, by making the best possible descriptions of these languages available to the linguistic community as a whole. This is literally a task that no one else can do, certainly one that no one else will do. ■

ERRATA AND ADDENDUM*
TO
Notes on Linguistics Number 40 October 1987

In Barbara Grimes article HOW BILINGUAL IS BILINGUAL? on page 8, the first paragraph, end of last sentence reads:

'..., as the proficiency level descriptions for Level 3 and 4.'

but, it should read:

'..., as the proficiency level descriptions for Level 3 and Level 3+ show.'

We apologize for any misunderstanding that this unfortunate typo may have caused. The correction should help clarify the crucial difference between marginal and adequate comprehension, which was the author's original intention.

Also, there was an obvious error in the authorship of the article THE SIL SECOND LANGUAGE ORAL PROFICIENCY EVALUATION. It was jointly-authored by the whole group (including Barbara F. Grimes) at the workshop, acknowledged in Ted Bergman's 'Preface.' We regret any embarrassment or confusion that our mistake may have caused.

*As an addition to THE SIL SECOND LANGUAGE ORAL PROFICIENCY EVALUATION article, in this issue (*NL*, 41) on the inside of the back cover, we include a reproduction of the scoring grid which was referred to on page 43 (*NL*, 40) under the heading 'Scoring the Assessment'. ■

RELATIONAL GRAMMAR: AN UPDATE REPORT *

Stephen A. Marlett

Relational Grammar is a syntactic framework which has been taught at SIL–UND for about a dozen years. In my opinion, the reason why Relational Grammar has remained the formal syntactic theory taught at UND during this time stems primarily from the insights it has given in a wide ranging variety of language types, including those of North America which are more synthetic in nature. This is not to say that it has nothing to offer to more analytic languages — witness recent work on French by Legendre (Legendre 1986), and on Italian by Rosen (Rosen 1981), for example, but it has had little impact on the study of Otomanguean languages such as Mixtec and Zapotec. (Similarly, earlier versions and later versions of generative grammar have had little impact on the description of these languages to date; these languages simply do not seem to provide a significant testing ground between syntactic models, although that may change.)

Relational Grammar has also been taught because it is a theory of clause structure which is relatively simple conceptually. At the end of the summer session, students do not know everything, of course, but the major lack is experience and practice, not concepts. And although relatively simple conceptually, the theory makes innovative claims which are not found in traditional grammar, Tagmemics, Government Binding theory, etc. Therefore it is more than just a handy way to catalog data. Thomas Wasow (1985:199) makes the following statement in his discussion of various current syntactic frameworks: 'Though it is rarely acknowledged, the influence of Relational Grammar on all contemporary work in syntactic theory would be hard to overestimate... [other theories] have devoted considerable energy to describing phenomena and capturing generalizations first discovered by relational grammarians.'

After the first few years of development of Relational Grammar, when early hypotheses were being formulated, tested, and discarded or revised quite rapidly, there have been few major changes in the theory. This fact makes it easier for fieldworkers to keep up on what's happening and absorb those changes which do take place.

The theory is not static, however, which is a sign that the theory is taking account of more data and attempting to improve on the accounts provided of data already described in some way. Because new work is constantly being produced within this framework (see, for example, the updated Dubinsky & Rosen bibliography recently published, indexed for language and topic), I have tried to provide former students from our second year grammar course with an annual or semiannual annotated bibliography. This brief article is an attempt to do that for a more general audience and at the same time sketch out the theory in its most current form.

Relational Grammar is built around two central ideas: grammatical relations and syntactic levels. Grammatical relations of major importance to the theory are *predicate (P)*, *subject (1)*, *direct object (2)*, *indirect object (3)*, various oblique relations with direct semantic correlates such as Locative and Instrumental, and *Chômeur (Cho)* (explained below). The idea of syntactic levels is an approach to the old issue of 'deep' vs. 'surface', 'logical' vs. 'grammatical', 'semantic' vs. 'syntactic' structures.

This is illustrated by the use of a passive clause in English such as 'Our mailman was bitten by a dog' (ignoring the issues raised by the auxiliary verb). The diagram in (1) utilizes a more compact representation than that used in most Relational Grammar work.

(1)	First Level	2	P	1
	Second Level	1	P	Cho
		Our mailman	*was bitten*	*by a dog*

The noun phrase 'our mailman' is claimed to be the direct object in the first, or 'initial', level of the clause. This grammatical relation in the initial level relates fairly directly with the semantic role of 'patient' or 'undergoer', which information is not utilized by the syntax. This noun phrase is claimed to simultaneously be the second level, in this case the 'final' level, subject. As final subject, it appears before the verb, determines agreement on the verb, is replaced by a nominative case pronoun, etc. — characteristics of final subjects in English.

The noun phrase 'a dog' is the initial subject, which relates fairly directly with the semantic role of 'agent' or 'actor'. The subjecthood of this nominal is usurped by the noun phrase 'our mailman' in the second level, however, and therefore by laws of clause structure which are proposed in the theory, 'a dog' is a Chômeur (French for 'unemployed') in the final stratum. In English, Chômeurs which are subjects in earlier strata are marked with the preposition 'by'.

This analysis of passive clauses provides a key to the understanding of switch-reference marking in Seri, a language of northern Mexico (see Marlett 1984a 1984b for more details). An adverbial clause is marked with a certain morpheme if its subject is different from the subject of the clause in which it is embedded (usually the one immediately following it). Unlike in other languages with switch-reference systems, however, the subject which is relevant is not the final subject of the passive clause, but the initial subject, even though this subject never appears overtly in the clause. An example is given in (2).

(2)	ʔap	kiʔ	t-oXl	ma	yo-p-aʔit.
	deer	the	died	diff=subj	it.was.eaten
	When a deer died,				*it was eaten.*

The second clause is passive (indicated by the prefix p-), and the final subject of both clauses is ʔap 'deer' (see the references above for arguments supporting this claim). And yet the 'different subject' marker appears at the end of the adverbial clause. The fact that the initial subject of the second clause is some nominal other than ʔap 'deer' is crucial to the understanding of switch-reference marking in Seri. An appeal to semantic roles such as 'actor' and 'undergoer' would not allow for any straightforward account of the facts. The relevant nominal in the first clause of (2) happens to be an undergoer, but it could have been actor and the switch-reference facts would come out the same.

A claim of Relational Grammar that has had more widespread appeal, given that it has been adopted in various ways by other syntactic frameworks, is that intransitive verbs (excluding impersonals such as 'rain') should be viewed as universally falling into two groups: those which occur with an initial subject and those which occur with an initial direct object. The former group typically includes verbs such as 'jump' and 'cry', and the latter verbs such as 'fall' and 'die'. The division usually falls roughly along the lines of voluntary vs. involuntary, but the crucial claim in Relational Grammar is that the distinction is syntactic as well as semantic. Example (3a) shows that the former group, referred to as unergative verbs, have only one level (initial=final), and 'John' is the subject. Example (3b) shows that the second group, referred to as unaccusative verbs, have two levels, and 'John' is the direct object in the first and subject in the second.

(3) a. First Level 1 P
John jumped.

b. First Level 2 P
Second Level 1 P
John slipped.

This division is not important in some languages, including (probably) English, but it has been shown to be of great significance in others, even though the division is not immediately obvious, such as Italian (Rosen 1981) and German (Perlmutter 1978). A language in which the distinction is very obvious is Madija, an Arauan language of Peru. (This discussion is based on work in progress by Patsy Adams, Pam Wright, and myself.) In this language some verbs obligatorily occur with an auxiliary verb, others never occur with an auxiliary verb. Basic transitive verbs and unergative verbs belong to the first group, and unaccusative verbs occur with the second group. The generalization appears to be that if the clause contains an initial subject, the auxiliary verb must be used. Three examples are given in (4). (Person agreement and tense occurs on the auxiliary verb when it is present.) Sentence (4a) has a transitive verb, which of course has an initial subject; therefore the auxiliary occurs. Sentence (4b) has an unergative verb, which has an initial subject; therefore the auxiliary occurs. Sentence (4c) has an unaccusative verb, which does not have an initial subject; therefore the auxiliary verb does not occur.

(4) a. dzabitso pa aba tsite inahari.
young.man – fish shoot 3–AUX–CMPL
the young man shot a fish (with an arrow)

b. la dza hoa hoa nabak^{h}ihari.
us to shout shout AUX–PL–CMPL
he shouted to/at us

c. onapiharo
1s–grow–CMPL
I grew

Failure to recognize the distinction between unergative and unaccusative verbs makes the grammar of Madija much more opaque.

Another major proposal which has had important implications for the study of various languages is that of multiattachment, where one nominal bears more than one grammatical relation in the same level, such as in reflexive clauses of various types. Rosen (1981), on Italian, and Berinstein (1984), on K'ekchi, are two important works in this area.

All of the topics discussed above join numerous others in Relational Grammar literature that appeared several years ago. So, one might ask, 'What is new?'

First, the research program has continued in looking for constraints on the distribution of grammatical relations in the clause. Bickford (1987) is one good example of this.

Second, new proposals have been made regarding sentences with auxiliary verbs, causative verbs, and causative prefixes. Davies and Rosen (in press) proposes that these sentences involve a sequence of 'predicate sectors'. I use the Madija sentence (4b) to illustrate in (5).

(5)

First Predicate Sector	3	1	P	
Second Predicate Sector	3	1	Cho	P
	ia	[poa]	hoa hoa	na
	us	he	shout	AUX

The first predicate sector has the verb **hoa** 'shout', which takes an initial subject and allows for an initial indirect object. This sector has only one level. The next predicate sector (required because the verb **hoa** has an initial subject, as discussed earlier) introduces the auxiliary verb na which does not itself take any arguments. The previously licensed arguments enter this sector with no change, but the predicate **hoa** loses its P relation, and bears the Chômeur relation in the second predicate sector. The final predicate in Madija carries the inflectional morphology; in (4b–5) the final predicate is the auxiliary and it therefore takes the inflectional suffixes **–bak^{h}i** (plural object, direct or indirect) and **–hari** (completive aspect).

This view of union clauses provides a solution to another problem of Madija grammar. Consider sentences such as (6).

(6) **toponi** **ti–na–hika–mana–hi**
outsider 2s–CAUS–be = finished–PL = SUBJ–IMPER
You (pl.) kill the outsider!

The clause has a direct object and a subject. The problem is: why doesn't the auxiliary verb occur with causative verbs? They are transitive verbs with initial subjects, aren't they? Under the Davies and Rosen view of union clauses, the answers fall out automatically. As shown in (7), the verb **hika** 'be finished' licenses the noun **toponi** in the first predicate sector, as a direct object. (**Hika** is unaccusative, consistent with its semantics.) In the second predicate sector the causative predicate (morphologically a prefix) enters, licensing its own subject, 'you' and making the verb **hika** bear the Chômeur relation in the second predicate sector.

(7)

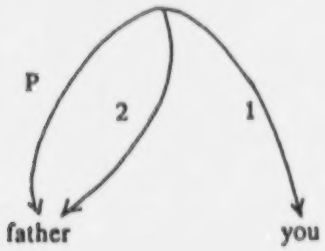
First Predicate Sector	2	P			
Second Predicate Sector	2	Cho	1	P	
	toponi	hika	[tia]	na–	
	outsider	be = finished	you	CAUS	

Therefore, although causative verbs are transitive verbs, the clauses do not have initial subjects under this analysis, and it is not surprising that they would not take the auxiliary verb.

Rosen is currently making a proposal for the internal structure of the noun phrase for those languages in which the head noun agrees with the possessor. She argues, based on data from Tzotzil, that it makes sense to view the head noun as simultaneously bearing the predicate relation and the direct object relation to the noun phrase node,

and the possessor as bearing the subject relation, as shown in (8) where I have to appeal to the 'arc' formalism which underlies the informal charts used above.

(8) Tzotzil: a–tot
2sERGATIVE–father
your father



I close this article with a list of books and articles. Relatively new works are indicated with an 'N' (for New) before them, for those who want to keep up with what is happening, and they are annotated briefly to give some idea of what is contained in them. Relatively basic works are indicated with a 'B' (for Basic), for those who want to know where to find information about the fundamentals of the theory and for librarians who want to know what items would be recommended for inclusion in a library. The remaining items have been cited in this article.

NOTES

* Thanks to Albert Bickford for commenting on an earlier draft.

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TONE AND STRESS ANALYSIS BY COMPUTER

by Geoffrey Hunt

0. INTRODUCTION

I have long been aware that it is possible to do acoustic analysis in a village situation, without having access to a specialist phonetics laboratory at a university. However, making this a reality has been a long process.

Some initial development work was done at a British university in 1977, then, in 1984, a project was started in Dallas. This was later handed over to JAARS, where Terry Gibbs made two boards that would fit into the back of an IBM PC or compatible. But Terry was forced off the project by pressure of other work.

In the summer of 1987 an opportunity arose to do the project in Britain, and we have been working on it since then. Philip Brassett has been the main developer; he got a friend to construct a board, again to go into the back of an IBM PC or compatible, and has been developing the software himself. The first two attempts to produce a graph of the basic tone were not satisfactory, but the latest attempt is much better, so that now seems a good time to let others know what is happening.

However, very few people in a village situation have a computer into which an extra board can be placed. So, now that the feasibility has been demonstrated, the next stage of the project is to refine the design of the board and to make it into an external box that will plug into the back of any computer that has a parallel port. Geoff Haydock is handling this part of the project.

1. TESTING AGAINST REAL DATA

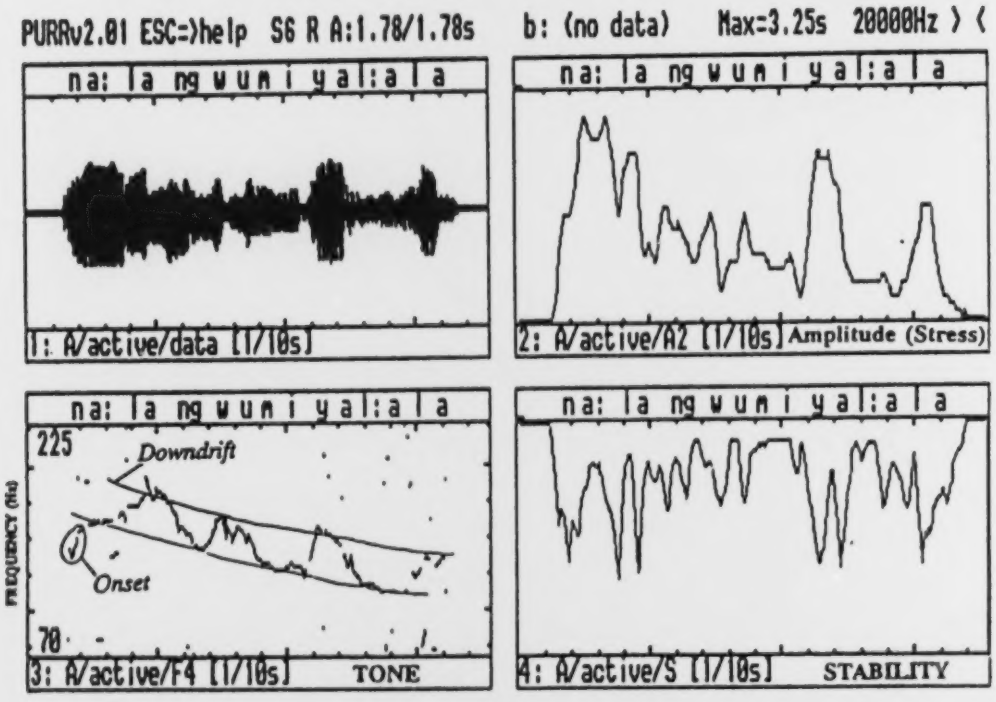
One problem in producing such a device for the analysis of stress and tone is the need to test it and see how closely the results tally with those produced by a good phonetician. Secondly, there might be a difference between perceived tone and actual tone, a problem that might be expected to show up with closed vowels, because of the weakness of the signal carried by the basic tone (fundamental frequency, or first harmonic) when compared with the strength of the signal carried by, say, the second harmonic.

My own field work took place in Ghana, among the Hanga people. The Hanga language is a tonal language, but my own ability to distinguish tone is very limited, which is why this project exists. Fortunately, my wife, Rosemary, is very good at hearing tonal distinctions, and she was responsible for checking all the tonal data that we used in our analysis. So I had some good data to check the computer analysis against. I initially took a short tape-recorded utterance, more or less at random, from each of two of our earliest texts; one was a young man telling a story, the other a girl giving instructions. A tape recorder was connected to the board in the computer and the speech sounds were digitized and stored in files in the computer.

2. OUTPUT PRODUCED BY THE COMPUTER

Let me focus on the first one of these digitized utterances. *Figure 1*, on the following page, shows the output obtained from the printer by doing a screen copy. A higher resolution printout may be obtained by letting the program handle the printing.

Figure 1. *Hanga*: Graphics printout of computer screen traces


PURRv2.01 ESC=)help S6 R A:1.78/1.78s b: (no data) Max=3.25s 20000Hz
na: | a ng w u m i y a l : a | a
1: A/active/data [1/10s]
2: A/active/A2 [1/10s] Amplitude (Stress)
225
Downdrift
Onset
70
FREQUENCY (Hz)
3: A/active/F4 [1/10s] TONE
4: A/active/S [1/10s] STABILITY

In this instance, there are four windows on the screen — it can vary between one and six. Each window refers to the same utterance (A), though it is possible to have the windows divided between two utterances. At the top of each window there is a rough representation of the phonetic script, reasonably well aligned with the sounds it represents. This can be done by selecting just a small part of the digitized sound with a vertical cursor and playing it back through the board, establishing where particular sounds begin and end.

Window one contains an averaged view of the digitized sound stream. The window was originally 3.25 seconds wide (64k of memory filled with one byte samples at 20,000 samples per second), but has been shortened to 1.78 seconds wide to focus in on the part to be examined. Slower sampling speeds may also be employed.

Window two contains a graph of the average amplitude of the signal. This is probably a fairly good approximation to the stress of the sound, though this has still to be tested. It will be seen to be similar to the shape of the envelope of the original sound. It is not quite the same, because different sounds can make the average shape of the original sound look bigger than the stress it represents.

Window three shows the basic tone of the digitized sound. The frequency range chosen for this particular speech stream was between 70Hz and 225Hz. Any frequency range between 70Hz and 400Hz could have been chosen.

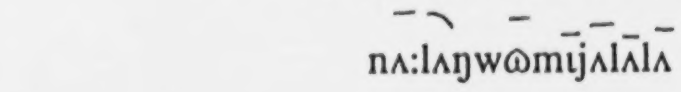
Window four shows the stability of the sound at different places in the speech stream. It can be imagined to be the inverse of the rate of change of sound, the peaks representing maximum stability, and the troughs representing maximum change. This is not useful information at the present stage, but could be useful if it turns out to be possible to do more thorough analysis of the speech stream.

3. COMPARISON WITH MANUALLY RECORDED PHONETIC DATA

Rosemary and I have analyzed the Hanga language as having two emic tones plus downdrift and intonation tone. If any two high tones have a low tone or have a downstep feature (probably a missing low tone) between them, the second high tone will not be as high pitched as the first; this is the meaning of downdrift.

Rosemary's analysis of tone as originally marked in our data book for text material is shown below. It agrees closely with that given by the computer, except for the very first syllable, which Rosemary had marked as being a relatively high pitch, whereas the computer indicated that it was a lower tone, rising more gently than the onset of the other high tones shown in *figure 1*, perhaps attributable to the lengthened vowel.

Figure 2. Text as originally transcribed:



Which analysis is correct? Rosemary and I had no problem realizing that the computer must be correct because the word involved (a shortened version of *naaba* 'chief') is basically a low tone word. So why did Rosemary hear it differently? The reason may well be shown in window 2, which indicates the stress of the utterance. The relevant syllable is the most stressed syllable in the utterance and, since stress in English (Rosemary's mother tongue) is associated with a relatively high pitch, the stress probably deceived her.

This output from the computer is very encouraging. The close vowels did not foul up the tonal analysis. The graph of tone against time agreed closely with that recorded by a good phonetician. The downdrift of tone showed up very clearly. A stressed low tone was indicated clearly.

When we considered the second Hanga utterance, the computer did not give such a clear graph of basic tone. But it was clear enough that the computer agreed completely with Rosemary's analysis. However, there is still a need to improve the algorithm, but this should be easily achievable by optionally using a more detailed, but slower, analysis.

4. OTHER LANGUAGES

We have not just looked at Hanga. The reason I have considered it in detail above is that both the original tapes and the original transcriptions were available, even though they were sixteen years old.

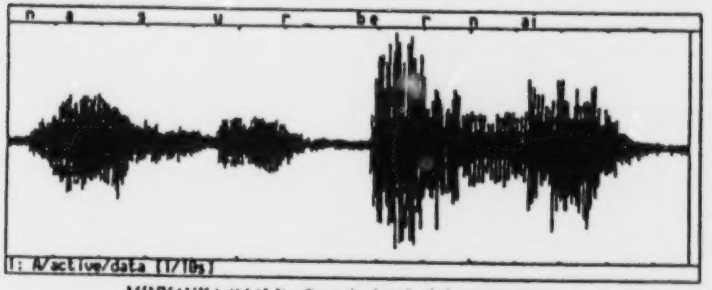
We have also looked at Nambiquara (Brazil), Cantonese, Minyanka (Mali), Arabic and Tamil. Nambiquara is a tonal language, but the computer shows that it is quite different from Hanga. Tamil, on the other hand, is a stress language, but it is difficult to analyze the stress. See *figure 3* on next page for some data from these languages.

5. OTHER THINGS THAT CAN BE DONE

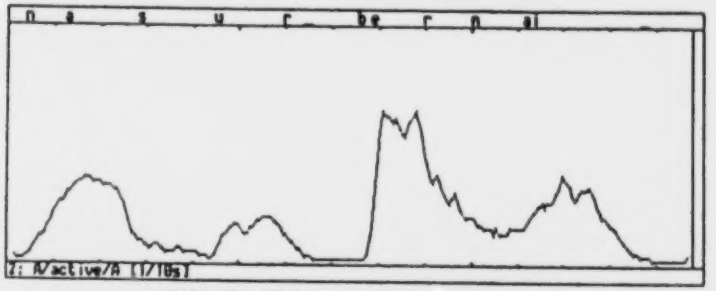
So far I have talked about how to find out what the stress and basic tone are doing, but there are a number of other things that can already be done.

By selecting a small portion of the digitized sound and playing it back through the loudspeaker on the board, just that part may be examined in more detail. By making several adjustments, the duration of a particular sound can be accurately determined.

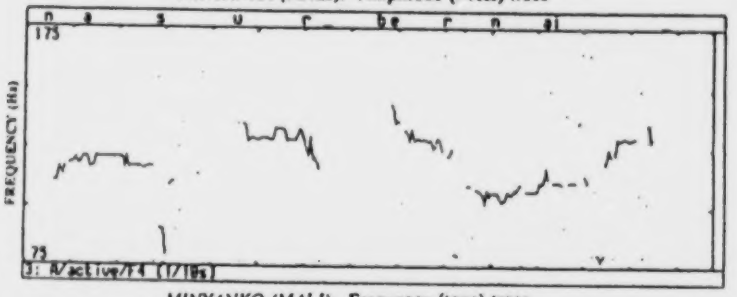
Figure 3. Other Languages: Graphics printout of computer screen



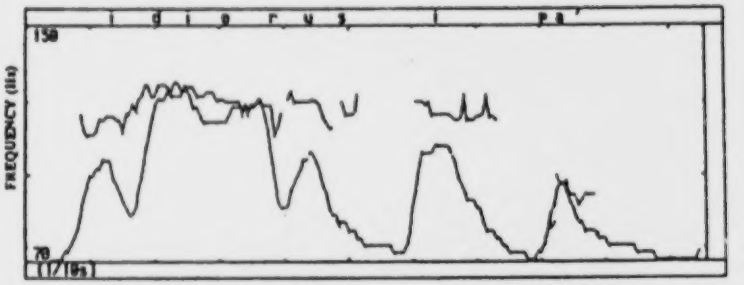
MINYANKA (MALI): Speech signal of short utterance



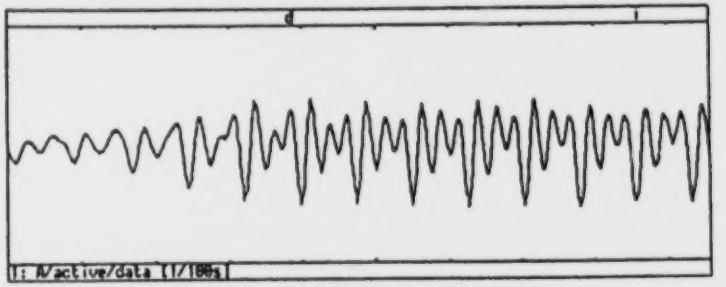
MINYANKA (MALI): Amplitude (stress) trace



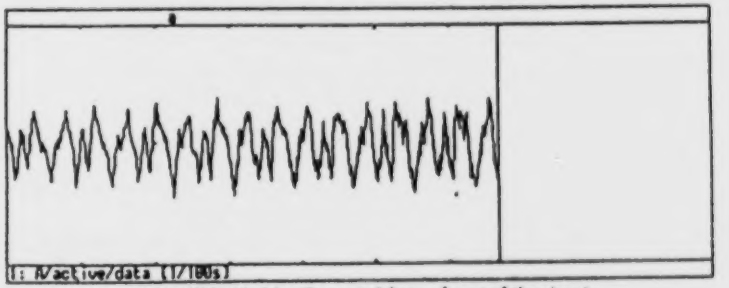
MINYANKO (MALI): Frequency (tone) trace



TAMIL: Tone (top trace) superimposed over Amplitude (stress)



TAMIL: Expanded form of part of the signal



MINYANKO (MALI): Expanded form of part of the signal

It is also possible to overlay two windows for purposes of comparison. For example, the stress and tone of a particular utterance can be viewed together, in order to see how dependent or independent they are from one another. In the same way the mimicked stress or tone may be compared with the original stress or tone, in order to improve speaking ability.

6. SOME TECHNICAL CONSIDERATIONS

The stress and tone analyzer works by first digitizing the sound from a tape recorder or microphone. This requires as many as 20,000 samples of the sound in one second. Now, in order to get this speed out of a bi-directional parallel port and save the data into memory as it goes, this part of the program has to be written in assembler or machine code. In addition, the address of the parallel port may vary from one computer to another. So, because machine code has to be used for addressing the port, different computers may require a modified form of the program.

Most computers now have a parallel port, usually to the Centronics standard, but many of these are not bi-directional; rather they are intended for use with printers, and may be called a parallel printer port. The Toshiba T1000 has a bi-directional parallel port, but it is not yet clear whether the T1100+ has. We have asked our local Toshiba dealer who has some information that says it should be, but no one knows how to activate it. The matter was referred to Toshiba (UK) who, in turn, referred it to Toshiba (Germany), and they referred it to Toshiba in Japan. We await an answer, but there may be another solution.

Even if a parallel port is not bi-directional, it may still be possible to transfer the digitized data through the port into the computer. This is part of the next stage of hardware development. We want to find the simplest device that will do the job for the greatest number of people in their field situations.

When Terry Gibbs produced his first board, each sample was eight bits. But he was told that this would not give sufficient resolution, and that a twelve bit resolution would be needed. In Britain we have been using an eight bit resolution and found that it is acceptable for almost all of the data we have tested. We cannot even be sure that increasing the resolution to twelve bits would improve the analysis. Since we want to keep the time and costs down for both the development and construction of these units, we have decided to stay with the eight bit implementation.

The unit we are now designing should have about fifteen integrated circuits, the largest having twenty pins.

Once the sound stream has been digitized and stored in the computer, the limitations on what may be done are set by the software. So we can expect ongoing development beyond that shown in this article.

7. A FINAL WORD

It is hoped that all hardware development for the present project will be completed by May this year. The software should also be available in an optimized form at about the same time. The problem will then be to get as many units made as are required. The cost per unit should be $200 or less.

Any correspondence relating to this development should be brief and to the point, and should be sent to Eugene Loos, the International Linguistics Coordinator in Dallas. ■

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CORRELATIONS BETWEEN

VOCABULARY SIMILARITY AND INTELLIGIBILITY

Joseph E. Grimes

On the face of it, it seems reasonable that dialects whose vocabularies are similar ought to be able to understand each other rather well. pet all too often they do not. In spite of that, decisions about language programs are sometimes made on the basis of this plausible sounding but shaky assumption.

At the low end of the scale there is a constant relationship: comprehension is always poor when vocabulary similarity is low. But that relationship does not hold up at the high end of the scale, which is where the program decisions have to be made.

The reason why high similarity is a poor predictor of high intelligibility is that there are other factors besides similarity in vocabulary that influence intelligibility. Even when vocabulary similarity is high, the other factors can get in the way — the effect of differences in function words and affixes, syntactic and morphological rearrangements, certain kinds of regular sound shifts, and semantic shifts in both genetically derived vocabulary and loans.

Because intelligibility is so complex, the hopes raised thirty years ago by glottochronology should have faded by now; yet they have not. From one or two hundred forms, some still would see linguistic and cultural history laid out, and would decide at a glance where the paths of communication lie open. One reason the hope stays alive is that it is thought to be more work to calibrate intelligibility accurately than it is to collect a word list and compare it with other word lists, so a short cut would appear to be welcome.[1] But the energy wasted on invalid short cuts could be better employed to give us surveys that are done right the first time.

Even when intelligibility tests are given, survey reports occasionally complicate the picture by confusing intelligibility with something quite different: what amounts to bilingual behavior on the part of some of the people tested. When people learn another language, even one that is thought of as a dialect of their own language but is different enough that they cannot treat it as a simple extension of their own mother tongue, all our testing has to be done differently.

The difference comes from the fact that when a community learns a second form of speech, each person in that community does so for his or her own reasons. Some don't feel they need it, and don't learn it; some would like to, but have no opportunity; most learn it well enough for their immediate ends. but no better. This means that bilingual proficiency within a community normally varies greatly from one person to another. The sample needed to test that variation has to cover all the segments of the society, because different social sectors take differently to learning another language or dialect.

Not so with inherent intelligibility. It is an extension of ability to use the mother tongue. As a consequence, what is accessible to one member of the community is accessible to all. Its range of individual variation is fairly narrow,[2] and a smaller sample is statistically adequate for estimating it.

So in reviewing how well or how poorly intelligibility might be predicted by vocabulary similarity, we do well to remember that when bilingual comprehension is reported as 'intelligibility,' we are really dealing with something whose distribution in society is quite different from that of intelligibility. In that case the degree of

understanding available to those who have not gone out of their way to learn the other form of speech is lower than the figure given as if it represented uniform intelligibility.[3]

Philippines

Vocabulary similarity estimates and intelligibility test results — subject to the cautions just given — are available for 55 pairs of dialects in the Philippines. In these pairs the intelligibility is only weakly correlated with the vocabulary similarity.

The data are from the Tenth Edition of the *Ethnologue* (B. Grimes 1984), reproduced in Table 1 and displayed in Figures 1 and 2. They are based on field surveys made by the Philippines Branch of the Summer Institute of Linguistics. The fractions of a percentage point given in the *Ethnologue* are rounded off here to reflect better the level of accuracy that tests of the kind given yield.[4]

Seven other dialect pairs were left out because the figures reported for them in the *Ethnologue* for intelligibility are known to involve a substantial amount of bilingualism, yet they come from tests on samples that were too small to be valid for the variation that goes with bilingualism. The languages involved are not even in the same linguistic subgroupings; on purely comparative grounds it would be strange if they understood each other inherently. Atta of Pamplona tested on Ilocano had a similarity of 63% and what was called 'intelligibility' of 85%; Itawit on Ilocano, 53% and 68%; Kasiguranin on Tagalog, 52% and 92%; Agusan Manobo on Cebuano, 81% and 88%; Obo Manobo on Cebuano of Nasuli, 35% and 78%; Central Tagbanwa on Cuyonon, 48% and 61%; and Central Tagbanwa on Tagalog, 40% and 54%. Ilocano, Tagalog, and Cebuano are used over wide regions of the Philippines, and Pilipino, based mainly on Tagalog, is taught in the schools.

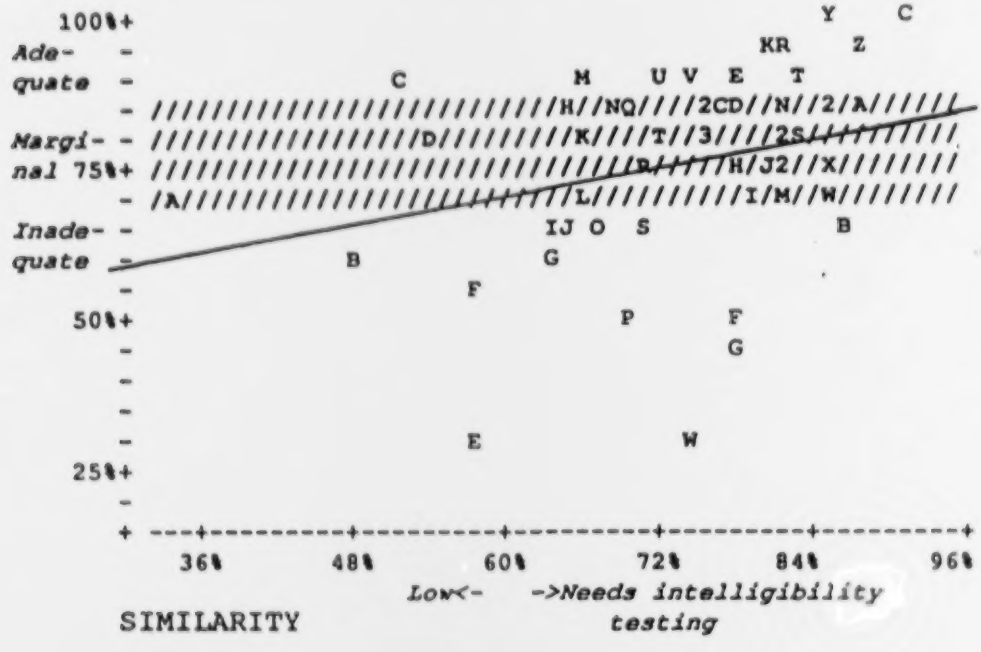
It is likely that unrecognized bilingualism is a factor in some of the other samples reported here as well. If it is, the degree of understanding of the part of the population that has not learned the other language very well is sure to be lower than the averages suggest.

There is also a question about how the vocabulary similarity figures were arrived at. In some cases — I do not know which — the figures probably represent the proven cognates that remain between two word lists after borrowing and internal analogies have haphazardly upset the smooth progress of sound change. Such cognate judgments would be based on the extensive studies of phonological comparison that have been made in parts of the Philippines. In most cases, however, I take the figures to represent proportions derived from impressionistic counts of words that appear to be phonetically similar, without any way to distinguish those that come from a single parent form at an earlier historical stage via demonstrable sequences of sound changes on the one hand, and loans and analogical formations on the other.[5]

Figure 1 shows how vocabulary similarity figures relate to intelligibility measurements in the Philippines. The fifty-five dialects in Table 1, from which Figure 1 is derived, are arranged from low to high similarity. They are identified with the letters A, B, C, and so forth for the first 26 on the list, corresponding to the left part of the figure, again as A, B, C,... for the next 26 going toward the right, and A, B, C for the last three on the extreme right. Where two or three dialects fall on the same point, a digit is given instead of a letter to show how many dialects are there. The graphs are unretouched output from the Minitab computer program, with clarifying annotations added.

Figure 1. Vocabulary similarity and Intelligibility in 55 dialect pairs in the Philippines

A,B,C... identify rows in Table 1, and are repeated after the 26th and 52nd dialects on the list.
Numbers are used where dialects appear on the same spot.
The zone of marginal intelligibility is shown by ////////////.
The regression line for the relation is shown by ————.



Normally (Simons 1979) vocabulary similarity percentages of 60% and below go consistently with intelligibility measured at 67% and below on simple narrative material. That level, for practical purposes, is inadequate for all but the simplest communication. Intelligibility seems to have to be above 85%, as measured on narrative, before much complex and personally revealing communication is likely to take place; Casad's discussion of Kirk's validation tests for Mazatec (Casad 1974.83–86) points to a 90% threshold for being able to extrapolate from a test on narrative to more complex kinds of communication.

It is clear from Figure 1 that similarity figures above Simons's 60% similarity line go with a wide range of intelligibility: from 31% (W) in the lower right to 98% (C) in the upper right. They are evenly balanced in that ten indicate adequate intelligibility (90% and up) and ten indicate inadequate intelligibility (under 70%). What relation there is between vocabulary similarity (s) and intelligibility (I) is indicated by the slanting shaded line, whose equation is:

$$I = 0.465s + 41.8$$

But the scatter of the dialects away from the line, measured by a correlation of 0.34, shows that there is only a weak relation between the two scales.

The actual dialect pairs for the Philippines that are displayed in Figure 1 are given in Table 1, with the vocabulary similarity and intelligibility figures for each. They begin with the lowest similarity figures in order to make it easy to visualize how the corresponding intelligibility scores vary.

Table 1. Philippine dialect pairs, with vocabulary similarity and intelligibility data from the Ethnologue

	VOCAB SIMIL	INTELL	Dialect Tested on data from reference dialect
A	34	69	Bagobo on Tagabawa Manobo
B	48	61	Central Tagbanwa on Cuyonon
C	52	90	Agutaynon on Calamian Tagbanwa
D	54	78	Cuyonon on Tagbanwa
E	57	29	Central Tagbanwa on Lamane
F	57	56	Central Tagbanwa on Calamian Tagbanwa
G	63	60	Obo on Tagabawa
H	65	83	Madukayang on Balangao
I	65	66	Tagbanwa on Quezon Palawano
J	66	63	Yaga on Central Cagayan Agta
K	66	82	Mt. Iriga Agta on Mt. Iraya Agta
L	66	72	Mt. Iriga Agta on Central Bicolano
M	66	92	Kamayo on Surigaonon
N	68	83	Madukayang on Limos
O	68	66	Ambala on Botolan Sambal
P	69	52	Pamplona Atta on Itawit
Q	70	87	Butuanon on Kamayo
R	71	73	Aklanon on Hiligaynon
S	72	64	Ibatan on Itbayatan Ivatan
T	72	78	Batad on Kiangan Ifugao
U	72	91	Piso on Kagan Kalagan
V	74	92	Mansaka on Kagan Kalagan
W	74	31	Ibatan on Basco Ivatan
X	75	82	Kasiguranin on Paranan
Y	75	85	Sibuco-Vitali on Balangingi Sama
Z	76	86	Mt. Iriga Agta on Iriga Bicolano
A	76	78	Karao on Ibaloi
B	76	81	Rajah Kabungsuan Manobo on San Miguel Calatugan Agusan
C	77	84	Lutungan on Balangingi Sama
D	78	87	Ayangan on Batad Ifugao
E	78	88	Hapao on Kiangan Ifugao
F	78	48	Tanudan on Butbut
G	78	47	SW Palawano on Central Palawano
H	78	76	SW Palawano on Quezon Palawano
I	79	70	Butbut on Guinaang Kalinga
J	80	77	Amganad on Kiangan Ifugao
K	80	94	Calamian Tagbanwa on Baras
L	81	81	Guinaang on Balbalasang
M	81	70	Guinaang on Limos
N	81	86	Madukayang on Mangali
O	82	74	Bangad on Butbut

P	82	81	Rajah Kabungsuan Manobo on Dibabawon
Q	82	76	Brooke's Point Palawano on Quezon Palawano
R	82	96	Brooke's Point Palawano on Central Palawano
S	83	81	Burnay on Amganad Ifugao
T	83	88	Brooke's Point Palawano on Southern Palawano
U	85	87	Mayoyaw on Batad Ifugao
V	85	83	Agusan on Dibabawon
W	85	69	Brooke's Point Palawano on SW Palawano
X	85	76	SW Palawano on Brooke's Point Palawano
Y	85	98	Palanan Dumagat on Paranan
Z	87	94	Casiguran Dumagat on Paranan
A	87	85	Hungduan on Kiangan Ifugao
B	87	63	Sindanga on Tuboy–Salog
C	91	98	Pamplona Atta on Northern Ibanag

Correlations

Table 2 and the condensed counterparts of it that are given in Table 3 for other data pinpoint a few areas for which there actually is a high correlation between vocabulary similarity and intelligibility over a part of the range. As is plain from Table 2, occasional areas of high correlation show up in very limited combinations of similarity and intelligibility, and comparing it with the tables given later shows that it cannot be predicted generally from one language area to another.

In Table 2 the data are the Spearman *r* correlations for all dialect pairs whose vocabulary similarity is greater than or equal to the threshold percentage given at the bottom of the column, and whose intelligibility test results at the same time are greater than or equal to the threshold given at the left of the row. The correlations are given to two decimal places in the (a) part of Table 2, and in a condensed form of one digit with the decimal point removed in the (b) part, which is also the format of Table 3.

The asterisks (*) represent those parts of the table for which there are fewer than 5 pairs available. From fewer pairs it is impossible to calculate a meaningful correlation.

The table focuses on correlations in the area of interest, from 60% and up for vocabulary similarity and from 70% and up for intelligibility. No figures are given for 95% and up; at that level there are not enough instances in any of the data sets to produce a correlation. An additional row and column have been added in order to include the full range of levels in the table.

The lower left corner cell (0,0) of Table 2 gives the correlation for all the data. In effect it measures the entire scatter away from the line of regression that Figure 1 shows: 0.34.

If we look only at that part of the data where Simons expects to find a useful level of intelligibility, 60% lexical similarity and higher and 70% intelligibility and higher (the 60% column with the 70% row), the scatter is relatively greater and the correlation measure is closer to zero: 0.21.

The other figures in the table show what the scatter would be if we were to cover up the lower part of Figure 1 so as to focus only on higher levels of intelligibility, and cover up the left hand part so as to focus on higher levels of vocabulary similarity, calculate the equation for another regression line like the one that crosses Figure 1 at an angle, but that covers only the points that have not been covered up, then calculate

how much those data points deviate from what the new equation predicts they should be. For example, if we look only at those pairs of dialects whose similarity measures are 65% or more (the 65% column in Table 2) and whose intelligibility measures are 80% or more (the 80% row in Table 2), the correlation measure that indicates how well the equation predicts the facts is 0.31.

Perfect correlation would have been registered if all the data in a plot like Figure 1 had fallen on a straight line instead of scattering out all over the plot as they have. In Table 2 perfect correlation would be expressed by a figure of 1.00.[6] Coefficients of correlation below 0.50 indicate considerable scatter of points off the regression line and are unimpressive in arguing for a consistent relationship between two variables; correlations below 0.30 indicate a still vaguer relationship, more scatter than the 0.34 that Figure 1 demonstrates. The computer program that calculated the 1,953 correlations that went into Tables 2 and 3 is reproduced as an appendix.[7]

Table 2. Philippine dialect pairs, correlations of vocabulary similarity and intelligibility from the Ethnologue

All dialect pairs whose similarity is equal to or higher than the figure at the bottom, and whose intelligibility is equal to or higher than the figure at the left, are correlated:

(a) to two decimal places, (b) condensed

(a)

PHILIPPINES.DATA

Intelligibility >= I%								
90%	0.85	0.83	0.83	0.85	0.52	0.52	*.**	*.**
85%	0.22	0.30	0.30	0.41	0.54	0.24	0.37	*.**
80%	0.19	0.31	0.31	0.28	0.49	0.37	0.48	*.**
75%	0.16	0.18	0.18	0.24	0.40	0.40	0.53	*.**
70%	0.14	0.21	0.21	0.24	0.36	0.46	0.53	*.**
0%	0.34	0.29	0.25	0.23	0.23	0.19	0.34	*.**
	0%	60%	65%	70%	75%	80%	85%	90%

Similarity >= S% for 55 pairs

* indicates N < 5, too few to correlate

(b)

PHILIPPINES.DATA(55)

Intelligibility >= I%								
90%	8	8	8	8	5	5	*	*
85%	2	2	2	4	5	2	3	*
80%	1	3	3	2	4	3	4	*
75%	1	1	1	2	4	4	5	*
70%	1	2	2	2	3	4	5	*
0%	3	2	2	2	2	1	3	*
	0	6	6	7	7	8	8	9
	0	0	5	0	5	0	5	0 %

Similarity >= S%

0 to 9 R = .0 to .9

The overall correlation improves in places as some higher levels of vocabulary similarity and intelligibility are considered, but at best it indicates a loose relation, not one with high predictive value. There is an exception in the 90% row of the table when similarity values lower than 75% are taken into account; suddenly everything appears closely correlated. The data in question are the top three rows of Figure 1, those pairs with intelligibility 90% or higher, which can be seen to fall fairly close to a straight line. The dialects involved, those in the upper left of Figure 1, are probably showing the effects of bilingualism. No analogous localized pocket of high correlation shows up in any of the ten other sets of data investigated in this way. It therefore appears to be a local fluke that can be attributed to a few situations where bilingual behavior was not recognized; it is probably not the manifestation of any principle.

Other language areas

Similar pairings of figures are available in Simons's monograph for ten other areas of the world. As with the Philippines, in most cases it is not possible to know whether the vocabulary similarity figures are based on counts of genetically demonstrable cognates or on apparent phonetic similarity alone, nor can we be sure that the intelligibility tests were not applied to bilingual behavior by mistake. Some of the intelligibility testing was done before the internal safeguards described by Casad (1974) were developed: the Iroquois tests, for example, were the first ever given.

The data are taken from Simons, who also discussed the sources and their quality. In order to show how vocabulary similarity and intelligibility correspond or fail to correspond in general I have analyzed his data both with and without various adjustments he proposes. With one exception, the adjustment factors influence the overall picture hardly at all.

One of his adjustments is for discrepancies greater than 10% in intelligibility measurements made in two directions, village A tested on village B and village B tested on village A. Intelligibility scores are almost always asymmetric in this respect. Simons suggests that discrepancies greater than 10% are due to social factors rather than linguistic factors. Since the social factors he refers to are more or less equivalent to the bilingual learning I said sometimes takes place between closely related dialects, a threshold on the order of the 10% he suggests is one way to recognize those factors tentatively, though its magnitude needs to be validated in areas like Spanish vs. Portuguese and in Chinese dialects, where difficulties in intelligibility can be traced to specific areas of phonology. (In the data from the Philippines given in Table 2, none of the intelligibility data report tests given in two directions. It is possible that only the higher score for a pair of tests was reported.)

The adjustment Simons makes for asymmetry is to exclude the pair of dialects with the higher intelligibility score, reasoning that the lower score is less likely to reflect a bilingual learning factor. In the data marked 'exclude' I follow his practice.

A major difference between Simons's correlations and mine, however, is that he includes the measure of a dialect on itself — often called the 'home town' measure — among the data to be correlated, and I do not. Discrepancies between home town scores actually measured and the 100% we might expect are part of the information needed in order to calibrate the test itself, but they are not part of the statement of the problem I am addressing. Even where the test results average below 100%, the effect of including the home town scores is a considerable increase in the correlations. I have therefore left all home town scores out of the calculations.

Because the earlier test designs (including the ones Casad reports) had not eliminated the sources of low home town scores,[8] it was thought necessary to adjust average scores by applying a correction factor based on the low home town score. Two kinds of corrections were applied. If it was assumed that speakers learned to take the test by the time they had finished the home town part of it, the home town score could be safely taken as not distinct from 100%, and the other scores could be left alone. Simons applies this kind of correction to his data for Biliau, Ethiopia, Mazatec, Siouan, and Trique. In Table 3 the results of adjustment for these are the same as for the basic data.

If on the other hand it was assumed that the difficulty carried over from one test into another, then the home town score was treated as equivalent to 100% and others were adjusted up in proportion to it. It is corrections of this form that Simons applied to

Buang, Iroquois, Polynesia, Uganda, and Yuman. In Table 3 the results of adjustment for these are too small to notice.

Adjustments of this kind ought not to be applied willy-nilly to data from other parts of the world without independent proof that conditions sufficient to justify them hold there; often they do not. Improved criteria for test construction have for practical purposes eliminated the need for adjusting scores.

I therefore give in Table 3 three kinds of summaries of the correlation ranges in Simons's data as he gives them in his Appendix 1. The first, tagged as *DATA*, has no special adjusting factors applied. In that form the summaries are typical of the information that becomes accessible during the course of many language surveys at a stage before some of the possible adjusting factors can be estimated.

Second, for tests where other scores are adjusted proportionally to the change made in the home town score, data from Simons's 'adjusted intelligibility' column are used in tables tagged as *ADJUST*.

Finally, in the tables tagged with *EXCLUDE*, I exclude from the computation the scores he marks with 'X' because they are more than 10% higher in intelligibility than the score going the other way. The small tables that make up Table 3 are the (b) or condensed form of the tables that were explained in connection with Table 2. Each adds in parentheses the total number of dialect pairs available when all levels are taken into account.

The negative *r* correlations in Buang, Polynesian, and Siouan indicate a reverse relation: the higher the similarity, the worse the intelligibility, within the thresholds given. Negative correlations are shown as minus signs in Table 3.

Table 3. Correlation ranges in ten language areas.
Data from Simons 1979, Appendix 1

Tables are in the condensed (b) format of Table 1.
• indicates fewer than 5 pairs, insufficient to correlate.
– indicates negative correlation
DATA unadjusted.
ADJUST adjustment based on home town score
EXCLUDE exclusion of asymmetries over 10%

BILIAU.DATA (6)

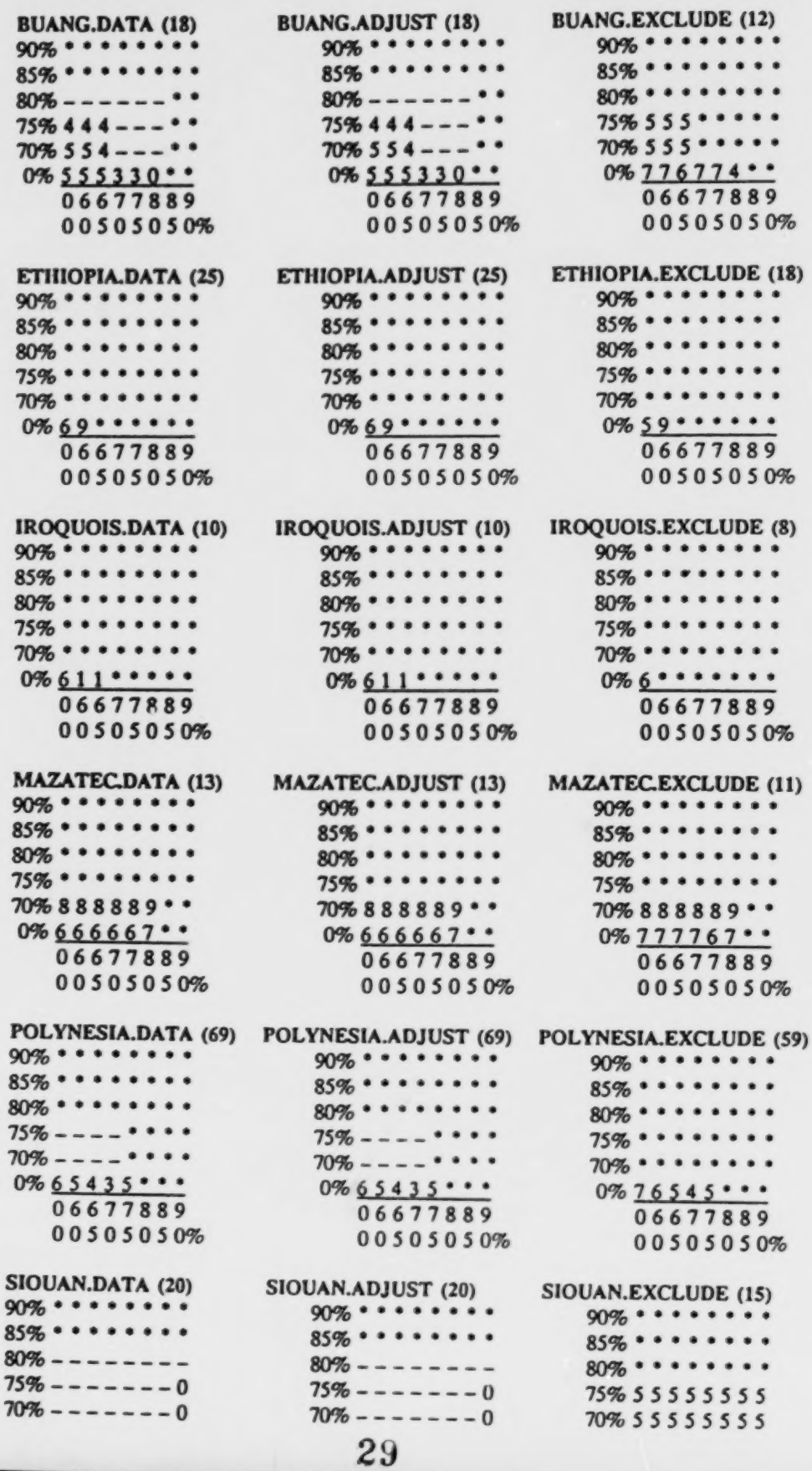
Intelligibility I%
90% • • • • • • • •
85% 2 2 2 2 2 2 • •
80% 3 3 3 3 3 3 • •
75% 3 3 3 3 3 3 • •
70% 3 3 3 3 3 3 • •
0% 3 3 3 3 3 3 • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
Similarity S%
0 to 9 R = .0 to .9

BILIAU.ADJUST (6)

Intelligibility I%
90% • • • • • • • •
85% 2 2 2 2 2 2 • •
80% 3 3 3 3 3 3 • •
75% 3 3 3 3 3 3 • •
70% 3 3 3 3 3 3 • •
0% 3 3 3 3 3 3 • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
Similarity S%
0 to 9 R = .0 to .9

BILIAU.EXCLUDE (3)

Intelligibility I%
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% • • • • • • • •
70% • • • • • • • •
0% • • • • • • • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
Similarity S%
0 to 9 R = .0 to .9


BUANG.DATA (18)
90% • • • • • • • •
85% • • • • • • • •
80% – – – – – – – • •
75% 4 4 4 – – – • •
70% 5 5 4 – – – • •
0% 5 5 5 3 3 0 • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
BUANG.ADJUST (18)
90% • • • • • • • •
85% • • • • • • • •
80% – – – – – – – • •
75% 4 4 4 – – – • •
70% 5 5 4 – – – • •
0% 5 5 5 3 3 0 • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
BUANG.EXCLUDE (12)
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% 5 5 5 • • • • •
70% 5 5 5 • • • • •
0% 7 7 6 7 7 4 • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
ETHIOPIA.DATA (25)
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% • • • • • • • •
70% • • • • • • • •
0% 6 9 • • • • • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
ETHIOPIA.ADJUST (25)
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% • • • • • • • •
70% • • • • • • • •
0% 6 9 • • • • • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
ETHIOPIA.EXCLUDE (18)
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% • • • • • • • •
70% • • • • • • • •
0% 5 9 • • • • • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
IROQUOIS.DATA (10)
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% • • • • • • • •
70% • • • • • • • •
0% 6 1 1 • • • • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
IROQUOIS.ADJUST (10)
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% • • • • • • • •
70% • • • • • • • •
0% 6 1 1 • • • • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
IROQUOIS.EXCLUDE (8)
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% • • • • • • • •
70% • • • • • • • •
0% 6 • • • • • • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
MAZATEC.DATA (13)
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% • • • • • • • •
70% 8 8 8 8 8 9 • •
0% 6 6 6 6 6 7 • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
MAZATEC.ADJUST (13)
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% • • • • • • • •
70% 8 8 8 8 8 9 • •
0% 6 6 6 6 6 7 • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
MAZATEC.EXCLUDE (11)
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% • • • • • • • •
70% 8 8 8 8 8 9 • •
0% 7 7 7 6 7 • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
POLYNESIA.DATA (69)
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% – – – – • • • •
70% – – – – • • • •
0% 6 5 4 3 5 • • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
POLYNESIA.ADJUST (69)
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% – – – – • • • •
70% – – – – • • • •
0% 6 5 4 3 5 • • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
POLYNESIA.EXCLUDE (59)
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% • • • • • • • •
70% • • • • • • • •
0% 7 6 5 4 5 • • •
0 6 6 7 7 8 8 9
0 0 5 0 5 0 5 0%
SIOUAN.DATA (20)
90% • • • • • • • •
85% • • • • • • • •
80% – – – – – – – –
75% – – – – – – – 0
70% – – – – – – – 0
SIOUAN.ADJUST (20)
90% • • • • • • • •
85% • • • • • • • •
80% – – – – – – – –
75% – – – – – – – 0
70% – – – – – – – 0
SIOUAN.EXCLUDE (15)
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% 5 5 5 5 5 5 5 5
70% 5 5 5 5 5 5 5 5

```
0% 7777776 0
   0667788 9
   0050505 0%
```

```
0% 7777776 0
   0667788 9
   0050505 0%
```

```
0% 8888887 0
   0667788 9
   0050505 0%
```

TRIQUE.DATA (11)

```
90% • • • • • • • •
85% • • • • • • • •
80% 6 6 6 6 6 5 • •
75% 6 6 6 6 6 5 • •
70% 7 7 7 7 7 5 • •
 0% 6 6 6 6 6 5 • •
    0667788 9
    0050505 0%
```

TRIQUE.ADJUST (11)

```
90% • • • • • • • •
85% • • • • • • • •
80% 6 6 6 6 6 5 • •
75% 6 6 6 6 6 5 • •
70% 7 7 7 7 7 5 • •
 0% 6 6 6 6 6 5 • •
    0667788 9
    0050505 0%
```

TRIQUE.EXCLUDE (7)

```
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% • • • • • • • •
70% • • • • • • • •
 0% 8 8 8 8 8 8 • •
    0667788 9
    0050505 0%
```

UGANDA.DATA (8)

```
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% • • • • • • • •
70% • • • • • • • •
 0% 8 7 • • • • • •
    0667788 9
    0050505 0%
```

UGANDA.ADJUST (8)

```
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% • • • • • • • •
70% • • • • • • • •
 0% 8 7 • • • • • •
    0667788 9
    0050505 0%
```

UGANDA.EXCLUDE (7)

```
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% • • • • • • • •
70% • • • • • • • •
 0% 9 9 • • • • • •
    0667788 9
    0050505 0%
```

YUMAN.DATA (20)

```
90% 4 4 4 4 4 4 4 •
85% 3 3 3 3 3 3 3 3
80% 2 2 2 2 2 2 2 4
75% 2 2 2 2 2 2 2 4
70% 5 5 5 5 5 5 5 4
 0% 9 9 5 5 5 5 5 4
    0667788 9
    0050505 0%
```

YUMAN.ADJUST (20)

```
90% 4 4 4 4 4 4 4 •
85% 3 3 3 3 3 3 3 3
80% 2 2 2 2 2 2 2 4
75% 2 2 2 2 2 2 2 4
70% 5 5 5 5 5 5 5 4
 0% 9 9 5 5 5 5 5 4
    0667788 9
    0050505 0%
```

YUMAN.EXCLUDE (16)

```
90% • • • • • • • •
85% • • • • • • • •
80% • • • • • • • •
75% • • • • • • • •
70% 9 9 9 9 9 9 9 •
 0% 9 9 9 9 9 9 9 •
    0667788 9
    0050505 0%
```

Of the eleven sets of scores presented in Tables 2 and 3, most of the data that contribute to high correlations come in the area of low vocabulary similarity predicting low intelligibility. To the extent that adjustments based on hypotheses about how home town scores work and exclusions of large asymmetries make any difference at all, they also make it mostly in this same area.

Higher on the scale, where marginal intelligibility is involved, two sets of scores out of the eleven, those for Mazatec and Trique, show a strong overall correlation (taking the pocket of high correlations with adequate intelligibility in the Philippines as something with no parallel anywhere else). Yuman has a stronger correlation than Mazatec in the area of marginal intelligibility, but only when asymmetries are excluded.

It may be significant that Mazatec and Trique comparative linguistics is fairly well advanced. Gudschinsky, who studied Mazatecan, and Longacre, who studied Trique, had both been in vigorous debate with Morris Swadesh about the validity of lexical similarity measures in general. They and Paul Kirk, who did the Mazatec survey with Casad, commanded both a working knowledge and a comparativist's broad grasp of the dialects that went far beyond what might turn up by the luck of the draw on a survey word list; they knew about cognates that might never be noticed without the detailed comparative work they had already done.

Discussion

To the extent that the areas for which we already have data on vocabulary similarity and intelligibility represent dialect areas in the world in general, chances are 4.5 to 1 against any survey that attempts to assess intelligibility solely on the basis of vocabulary similarity being able to do so with any confidence, on the basis of 9 areas of low correlation against 2 of higher correlation.[9] One has to do the intelligibility testing anyway, not only because it is the best indicator we have of areas of high communication potential, but in a secondary sense in order to validate whether the dialect area might indeed be one of the less likely ones in which vocabulary similarity bears a significant relation to comprehension.

This does not mean that we abolish the use of word lists in language surveys. It means instead that we no longer try to squeeze out of them information they are inherently incapable of giving. They do show up areas where intelligibility is unlikely, the ones where similarity is below 60%. Above that, counts based on them are helpful mainly to point up the need for intelligibility testing, but they are not a substitute for it.

Word lists should be used instead — especially now that we have rapid methods for establishing consistencies in sound correspondence — to give an initial picture of language groupings based on shared innovations in sound change, and to show the specific sound changes that result in those groupings. For such groupings, based on demonstrable genetic divergence, we can if we like quantify the conclusions reached by the comparative method, whether through phonostatistical indices of divergence,[10] groupings based on shared rules,[11] or computations of vocabulary similarity based on the retention of proven cognates under various conditions that encourage or discourage borrowing.

But even quantifications of full-fledged comparisons do not measure the other factors that are known to influence intelligibility. We have no comparable measures yet for calibrating morphological differences, syntactic differences,[12] or shifts of meaning, or for the social and geographic factors; nor if we had them would they necessarily combine meaningfully with a similarity index into a single composite figure that we could validate against measures of comprehension. For the present, however, we do have an effective strategy for arriving at decisions about language programs[13] that gives reasonable results unless we try to cut corners with it:

(1) Inspect word lists.

(2) If similarity is below 60%, assume separate programs.

(3) If similarity is 60% or better, test for intelligibility after screening subjects for possible bilingual learning of the other dialect.

(4) If intelligibility scores are uniform (standard deviation below 15%) and average scores are 85% and above, combined programs may be possible.

(5) If combined programs are linguistically possible, test social attitudes to make sure a combined program is feasible. The sampling requirements and the testing strategy for questionable cases are very different from the ones appropriate for testing for inherent intelligibility.

(6) If intelligibility scores are spread out (standard deviation 15% or greater), the problem becomes one of assessing what proportion of the population is at each level of bilingual proficiency. The sampling requirements and the testing strategy for determining this are very different from those appropriate for inherent intelligibility.

NOTES

1 Using word lists to study regularities in sound change is not a short cut comparison. It is, however, at least as time consuming and demanding as intelligibility testing, even with the aid of a computer.

2 Most surveys fail to report either this range of individual variation or the number of speakers tested in each place. Both are needed in order to interpret correctly the average intelligibility, which is the only one of the three essential figures that usually appears. The standard deviation is a useful measure of the range of variation. It is easy to calculate and is needed for statistical reasoning. It is gotten by taking the amount by which each individual's score deviates from the average, squaring it to keep the deviations that are below the average from canceling out those above it, adding up the squares of the deviations, dividing the sum by the number of test subjects to get the average squared deviation, then taking the square root of that to put it all back onto the original scale. Standard deviations for inherent intelligibility are normally less than 15% (a ball park figure, not yet validated precisely), while a larger standard deviation is typical of bilingual situations.

3 Casad, p. 177, gives a set of individual scores from the Mazatec survey. The scores for Huautla (Hu) break into three groups. Three people have scores in the fifties: I would guess they represent the real intelligibility. Four have scores 90 to 100: they could be the practiced bilinguals, though this kind of test cannot distinguish high fluency from only moderate proficiency. The other three are in between, possibly reflecting low proficiency in the Huautla dialect. Huautla is a market and cultural center whose speech is learned by people from the countryside. The mean intelligibility is 76%, but the standard deviation is 18%, enough of a scatter to raise suspicion.

4 Three significant digits gives a spurious impression of the accuracy that can be attained from the form of the test usually given. Rounding the community averages to the nearest 5% would reflect the inherent precision of that type of test even better than rounding to 1%. Tests and sampling procedures could be devised that would be accurate to 1%, but they would be extremely costly, and the decisions about language programs made on that basis would not be noticeably different from tests accurate to 5%.

5 There are, of course, many other dialect pairs in the Philippines for which neither similarity nor intelligibility figures have been compiled. The ones given represent dialects that were judged close enough to be worth testing. Most of the others fit into the low intelligibility, low similarity category mentioned in the first paragraph of the paper. They would fill the lower left quadrant of Figure 1.

6 Positive correlations go with regression lines that rise from left to right like the one in Figure 1. If the line fell, indicating inverse correlation (the more of this, the less of that), the correlation would be negative, and perfect negative correlation would be expressed as –1.0.

7 Only 1,448 correlations are actually given because the rows and columns that correspond to 95% were dropped from all the tables after they were calculated, since none contained enough data to give a valid correlation. Each correlation involved from 6 pairs of numbers for Biliau to 29 pairs for Polynesian.

8 The problems introduced artificially by adjusting intelligibility scores that fall below 100% for the subjects' home towns have been greatly reduced by stipulating that the only questions considered admissible for a text be those on which the panel of

home town speakers that Casad uses have a score of 100%. At the time he published his monograph it was still not clear that this would work. On pp. 61–62 he speaks of throwing out questions that half the speakers had difficulty with; the improvement has come from following through on this concept by throwing out all questions (out of a very large initial pool of possible ones) that the panel cannot answer well. In earlier testing, inability to answer questions in one's mother tongue had been thought to be due mainly to the unfamiliarity of the test situation, so the corrections proposed treated it as an error factor in learning that would diminishwith time and experience. For the argument I am making I have to assume that its noise effect is randomly distributed throughout these data, because we have no record of the order in which the test tapes used were presented to different subjects, and hence of how their responses might be weighted for learning behavior.

[9] The score is 8 to 3 if the Yuman data after exclusions are taken into account as well.

[10] See Grimes and Agard 1959 and Grimes 1964 for phonostatistical quantification.

[11] Suitable methods are described in Romesburg 1984, in sections on qualitative resemblance matrices.

[12] Andrew S. Noetzel and Stanley M. Selkow, and David Sankoff and Roger J. Cedergren, in two chapters in Sankoff and Kruskal 1983, lay the groundwork for tree-to-tree comparison measures.

[13] Barbara F. Grimes (1985) explains the rationale for this strategy.

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APPENDIX

Computer program in BASIC to calculate correlations.
Runs on the Sharp PC-5000, Kaypro 2000,
and other IBM PC compatibles.

```
  1  ' SIMINT – Correlate vocabulary similarity with intelligibility
  2  ' Joseph E. Grimes, 1986 October 13
  3  '
 10  LI=100 : LN=10 : L=60
 20  DIM SD(LI), ID(LI)
 30  DIM SL(LN), IL(LN), RROW(LN), NROW(LN)
 40  NS=9 : NI=7
 50  '
100  FOR I=1 TO NS : READ SL(I) : NEXT I    'Similarity threshholds
110       DATA 0,60,65,70,75,80,85,90,95
120  FOR I=1 TO NI : READ IL(I) : NEXT I 'Intelligibility thresholds
130       DATA 95,90,85,80,75,70,0
140  '
200  INPUT "File or device name for output";O$
210       OPEN O$ FOR OUTPUT AS #2
215       IF O$="LPT1" OR O$="lpt1" THEN PRINT#2,CHR$(27);"*1"
220  '
230  FOR Z=0 TO 1 STEP 0
235       IF O$<>"LPT1" AND O$<>"lpt1" THEN BEEP
240       INPUT "File name and extension [NNNNNNNN.XXX] for the data";F$
250       IF F$="" THEN CLOSE #2 : STOP
260            OPEN F$ FOR INPUT AS #1
270  '
300       FOR K=1 TO LI          'Read the data
310            INPUT #1, SD(K), ID(K)
320            IF SD(K)=0 AND ID(K)=0 THEN 350
330       NEXT K
340       K=K+1
350       K=K-1                                   'K pairs read in
360       CLOSE #1
370  '
400       PRINT#2,TAB(4);F$;TAB(5+NS*5+3+4);F$ : L=L-1
410       PRINT#2,"Intelligibility>=I%";                TAB(5+NS*5+3);
          "Intelligibility>=I%" : L=L-1
420       FOR I=1 TO NI
430            IN=IL(I)                    'By intelligibility thresholds
440            PRINT#2,USING"###% ";IN;
450            GOSUB 500                   'Correlate with similarity
460       NEXT I
470       GOSUB 900                        'Bottom of graph
480  NEXT Z                                'End of one data set
490  '
500  FOR J=1 TO NS              'Correlate with similarity (450)
510       SIM=SL(J)              'By similarity thresholds
520       GOSUB 600                              'Print the correlation
```

```
530       NROW(J)=N : RROW(J)=R'                    and save it for the graph
540   NEXT J
550   GOSUB 800                                     'Print the graph
560   RETURN
570   '
600   N=0 : SX=0 : SY=0 : XY=0 : X2=0 : Y2=0 'Correlation (520)
610   FOR M=1 TO K                  'Go through the data
620       X=SD(M) : Y=ID(M)
630       IF X<SIM OR Y<IN THEN 650 ELSE N=N+1
640       SX=SX+X : SY=SY+Y : XY=XY+X*Y : X2=X2+X*X :
          Y2=Y2+Y*Y
650   NEXT M
660   '
700   IF N<5 THEN R=0 : PRINT#2,"    *"; : GOTO 780
710   A=SX/N : B=SY/N : C=XY/N : D=X2/N : E=Y2/N
720   YX=XY-(SX*SY) : XX=A*A-D
730   BB=YX/XX                                      'Slope of regression line
740   AA=B-BB*A                                     'Intercept of regression line
750   R=(XY-(SX*SY)/N) / SQR(ABS(X2-(SX*SX/N))*ABS(Y2-(SY*SY/N)))
                                                    'Correlation
760   IF R>1 OR R<-1 THEN PRINT#2,"   ??"; : GOTO 780
770   PRINT#2,USING" #.##";R;
780   RETURN
790   '
800   PRINT#2,USING"   ##%";IN;                     'Print the graph (550)
810   FOR J=1 TO NS
820       IF NROW(J)<5 THEN PRINT#2," *"; : GOTO 850
830       IF RROW(J)<0 THEN PRINT#2," -"; : GOTO 850
840       IF RROW(J)>=0 THEN PRINT#2,USING" #";INT(RROW(J)*10);
850   NEXT J
860   PRINT#2,"" : L=L-1
870   RETURN
880   '
900   PRINT#2,TAB(10);                              'Bottom of graph (470)
910   FOR J=2 TO NS : PRINT#2,"———"; : NEXT J
920   PRINT#2,TAB(5+NS*5+6);
930   FOR J=1 TO NS : PRINT#2,"—"; : NEXT J
940   PRINT#2,"" : L=L-1
950   '
1000  PRINT#2,TAB(5);
1010  FOR J=1 TO NS : PRINT#2,USING"  ##%";SL(J); : NEXT J
1020  PRINT#2,TAB(5+NS*5+6);
1030  FOR J=1 TO NS : PRINT#2,USING" #";INT(SL(J)/10); : NEXT J
1040  PRINT#2,"" : L=L-1
1050  '
1100  PRINT#2,TAB(5+NS*5+6);
1110  FOR J=1 TO NS : PRINT#2,USING" #";SL(J)-INT(SL(J)/10)*10; : NEXT J
1120  PRINT#2," %" : L=L-1
1130  PRINT#2,TAB(12);"Similarity>=S% for"; K; "pairs"; TAB(5+NS*5+6+3);
      "Similarity>=S%"
1140  PRINT#2,TAB(5);"* N<5 too few to correlate  - R<0 reversed 0 to 9 R=.0
      to .9"
1150  PRINT#2,"" : PRINT#2,"" : L=L-4
1160  IF L-NI-8<=0 THEN L=60 : PRINT#2,CHR$(12)
1170  RETURN
1180  END ■
```

A SENTENCE-BASED METHOD FOR INTELLIGIBILITY TESTING

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0. Introduction
1. The standard text-test approach
2. The sentence-test approach
3. Conclusion

0. Introduction

The field of synchronic dialect surveying has blossomed in the last ten to fifteen years. The work of Bradley (1968), Casad (1974), Joseph Grimes (1979, 1988), Barbara Grimes (1988) and Gary and Linda Simons (1977, 1979), among others has outlined for field linguists what may become the standard paradigm for doing dialect survey work—we may be on the threshold of establishing widespread agreement among field linguists about the best methods for doing synchronic dialect research, especially regarding dialect intelligibility testing.

While the present methodology for intelligibility testing has proven useful in describing multi-dialect language situations, it has disadvantages which, to some extent, have hindered its wide-spread use as a dialect survey instrument. In this paper I outline a sentence-based intelligibility test approach which, in some situations, may retain the reliability and validity of the standard methodology while addressing some of its inherent problems.[1]

1. The standard text-test approach

Intelligibility testing has been used to determine how well certain communities understand the languages spoken in other communities since the early 1950s, when Voegelin and Harris (1951) used it as a means of investigating the genetic relationships between languages. While its use in diachronic linguistics has since been discredited (Wolff 1959), this form of testing has proven helpful in synchronic dialect studies, where the focus is on measuring current understanding, not genetic relationships.

Although a number of different approaches to intelligibility testing have been documented (Simons 1979:12), the most common methods share several characteristics. An investigator goes to a particular site in each dialect area to be investigated, called the reference site, and elicits a short, autobiographical story, which is tape-recorded. These tape-recorded stories are then mechanically adapted to produce a test tape which can be used to determine a subject's comprehension of each story. The adaptation involves dubbing content questions or pauses at regular, short intervals in each tape-recorded story.

All of the stories on the test tape are then played in various communities within each dialect area, called test sites, and the listeners' comprehension of each story is evaluated normally through asking content questions at regular intervals throughout the story. By totaling the number of correct responses given in each story, one can calculate how well people at a given test site understood the various dialects represented on the test tape. People who only answered 50% of the questions in a given story correctly are assumed to understand about 50% of the dialect spoken in that story, and so forth.

I will call this 'the *standard text–test approach* to intelligibility testing.'

There are several problems with this type of approach. An obvious logistical problem is the difficulty in eliciting useful stories to be used in the test. 'Tell me a story' is a notoriously hard request for an uneducated, untrained language helper to satisfy. This situation is compounded when the informant is dealing with a stranger, as the investigator often is in a survey situation. This difficulty is not insurmountable but it is inherent to this approach.

Another logistical problem is the difficulty of preparing the test tape. Mechanically adapting a tape–recorded story by dubbing in pauses, or, even more complex, dubbing in content questions in each local dialect, may intimidate people who are linguistically sharp but mechanically unsophisticated. Further, the statistical calculations typically associated with this methodology are even more intimidating to linguists who are not mathematically inclined. This has created a public relations problem for the standard text–test approach — to many field linguists it appears to be prohibitively complex.

A potentially serious problem with the text–test approach is the non–comparability of the texts used in the tests (Casad 1974:90). In other words, the story used to test the comprehension of 'Dialect A' may not be comparable in difficulty to the story used to test comprehension of 'Dialect B.' Speakers differ in their eloquent use of grammatical structures and in the richness of their vocabulary. The stories themselves will differ in content and in the semantic fields referred to. When stories are elicited whose only constraints are that their length be short and their content autobiographical, I do not think we are justified in assuming those texts reflect comparable cross–sections of the different dialects. There are simply too few controls.

Further, each text will use only a limited range of the grammatical features of the dialect being recorded, and only a few of the language's semantic fields will be referred to. As a result, the texts not only represent different cross–sections of the various dialects, but each text probably represents an unusefully small cross–section of that dialect. (Casad 1974:90)

A problem related to this is that in using the content question approach to evaluate comprehension, the investigator only has opportunity to test the comprehension of a fraction of the total content of the story. This shrinks the cross–sections being examined even further.

Finally, scores achieved in a text–test approach may reflect a higher degree of comprehension between dialects than is actually the case. In a text test, the listener is helped by context in answering the content questions. A listener who has only caught the gist of a story nonetheless stands a good chance at guessing the correct answer to a content question on the basis of what little he did understand, and what he knows of the culture and life in his home area. These factors indicate that text–test scores may reflect a higher degree of intelligibility than is actually the case. (Casad 1974:95)

Most of the problems mentioned above can be summarized as a failure to control potentially significant variables in the intelligibility test instrument. The sentence method I present below represents an attempt to control these variables, while at the same time retaining or increasing the reliability and validity of the standard text–test approach.

2. The sentence–test approach

Casad (1974:90, 95) recognized most of the disadvantages I have mentioned to using texts as the basis for intelligibility testing. In fact, although his confidence in the standard text–test approach was (and remains) unshaken, at one point he did suggest the same solution to these problems that I propose here — that we use ISOLATED SENTENCES as the basis for intelligibility testing, rather than coherent text material. In comparing the text–test method of intelligibility testing with the sentence–test approach, Casad (1974:98) states, 'the sentence test may be a more sensitive measure of intelligibility between dialects that the text test.' Below I present the *sentence–test approach* which I developed to survey the dialects of Yawa, a non–Austronesian language spoken in Irian Jaya, Indonesia.

To construct the sentence test tape, I elicited a set of 30 sentences at each reference site. A group of informants was asked to translate that set of sentences orally from the source language (SL), Indonesian, to the receptor language (RL), their local dialect. These sentences were spoken for tape–recording by a representative of the group for use in intelligibility testing.

The sets of sentences elicited at each reference site differed semantically but were comparable in the range of grammatical constructions found in each set. Each set of test sentences included at least one of each of the following constructions: a benefactive, a transitive clause, an intransitive clause, a locative, an expression involving temporal succession or duration, a cause–effect sentence, an instrumental, a conditional sentence, a command, a prohibition, an equative clause, a possessive, a negative, an information question, and a comparison. The grammatical constructions chosen for inclusion in the tests were those found to have distinctive morpho–syntactic marking in the dialect of Yawa with which we were already familiar. The lexical items in each test were from the same broad range of semantic domains, so that each test included a comparable, yet representative cross–section of speech in a given dialect. Perhaps most importantly, each of the elicitation sentences was carefully constructed so as to be easily and unambiguously translatable into the dialect of Yawa with which we were familiar, and also so that the resulting Yawa sentence was easily and unambiguously translatable back into Indonesian. As I will mention below, this step of careful construction of the elicitation sentences so as to be easily and unambiguously translatable both from the SL to the RL and vice versa, is critical to the validity of the method.

The actual testing procedure at each test site was as follows: a group of informants at each test site[2] listened to sets of sentences from each reference site dialect, beginning with their own home–town dialect. After each sentence, the group was asked to arrive at a consensus as to its meaning and to translate that meaning into Indonesian. These translations were then compared with the corresponding Indonesian elicitation sentences in order to determine the degree to which each sentence was correctly understood.

The method of scoring the tests was as follows: the first ten sentences of each test were discarded to allow for the informants' becoming accustomed to the new dialect being heard. The careful spread of semantic fields and grammatical constructions mentioned above was always contained in the latter 20 sentences of each test. The sets of original Indonesian elicitation sentences were then examined and the total number of basic meaning components (BMC) in each set of sentences was calculated. Each major lexical item (e.g. noun, verb, adjective, adverb and number) in each sentence was assigned the value of one BMC. The grammatical relationships between the various lexical items in a single sentence were collectively assigned the value of one BMC as well. Thus, for example, in the sentence 'I threw the plate into the sea,' the

lexical items *I*, *threw*, *plate*, and *sea* are each valued as constituting one BMC. An additional BMC is added as the value of the various grammatical and semantic relationships between these lexical items in the sentence, yielding a total of five BMCs for the sentence. The total number of BMCs in each set of Indonesian elicitation sentences was roughly 100. (See the appendix to this paper for some English translations of typical test sentences and their BMC counts.)

The original Indonesian elicitation sentences were then compared with the corresponding Indonesian translations given at each test site. Each translation of a test sentence was given one point for every BMC which matched a BMC in the corresponding original Indonesian elicitation sentence. All of the BMCs correctly identified in a given test were then totaled.

This total was then divided by the total number of BMCs in the original set of elicitation sentences to arrive at a percentage of the test which was correctly understood. It is assumed that the percentage of the test understood, as calculated by this method reflects the percentage of the reference site dialect which speakers of the test site dialect understand. This type of scoring and calculation was carried out for all the tests given at each test site.

There are many features of this method of intelligibility testing which could be quibbled with. The features which I consider most significant are first, that the test is based on sentences rather than texts, and second, that the format of the test was translation rather than content question.

The advantages of this method are several. First of all, the use of sets of sentences which all include the same range of grammatical devices and the same semantic fields of vocabulary increases the comparability of the tests elicited at the various test sites. As a result, the relationships between the intelligibility scores achieved on the various tests in the various test sites should be very accurate.

Secondly, the breadth of vocabulary and grammatical structures used in each set of test sentences should increase the absolute accuracy of the test scores. In other words, each test score should be a better representation of how much of the reference dialect the speakers of the test site dialect can understand, because each test contains a broader cross-section of the reference dialect to which the speakers of the test site dialect can respond.

Further, the mechanics of test construction and administration are not prohibitive. The elicitation and recording of test sentences consumed about one hour at each reference site, and about that same amount of time was needed to administer the test at each test site. There was no intermediate preparation of the test tape. Once it was elicited and properly recorded, it was immediately ready for use as a survey instrument.

Because the tests were comprised of isolated sentences referring to a broad range of semantic fields, there was no possibility of the subjects guessing the translation of a given sentence from context. Thus, the sentence tests gave a more accurate reflection of what subjects actually understood, as opposed to a reflection of what they could have surmised from context.

However, there are several limitations to the method I have outlined here. First of all, I find the scoring method somewhat crude and ad hoc. While I have tried to give all the relevant semantic and grammatical factors some scoring weight, I have no theoretical or empirical basis for the relative scoring of grammatical and lexical comprehension in the tests. This is an area that needs further work.

More troubling than the crudeness of the scoring is the fact that the test format depends on two translations: a translation from the SL into the RL to obtain the test sentence recording, then another translation back from the RL to the SL during the actual administration of the test. This type of procedure is fraught with problems. It depends in large degree on the bilingualism of the group of speakers whose dialect is under investigation. Obviously this method would not be viable in a monolingual situation.[3] Further, it is subject to the charge that what is really being tested is not comprehension but ability to translate — two skills which are related but not the same. A related objection to the sentence–test methodology is that it is subject to unmeasurable distortions due to its translation format to which the standard text–test approach is not subject, in that the standard text–test method depends on natural language data both in the formulation of the test instrument and in the test answers to be scored.

The main response to these objections lies in the investigator's careful choice of test sentences to elicit, based on his/her knowledge of both the SL and the RL. Even if the people being surveyed are only modestly bilingual, an investigator who is aware of the lexical and grammatical structures of both the SL and the RL should be able to construct a series of test sentences which are easily and unambiguously translatable in both directions. Sufficient care in the design of the intelligibility test instrument will minimize the failings inherent in the translation format of the test.

Another major objection to the translation format of this method is that it places unnecessary limitations on the test which may skew its results—the translation format limits the test to bilinguals and the principle of translatability limits the content of the test. However, the first limitation may not be as serious as it appears, in that what is to be measured in a dialect intelligibility survey is inherent intelligibility, that is a person's ability to understand a given dialect based solely on his knowledge of his mother tongue dialect. Theoretically this type of intelligibility should be fairly uniform across the various subgroups of a language community, in that each native speaker's ability in his mother tongue dialect should be about the same. This being the case, a test group composed of people who happen to be somewhat bilingual in a trade language not closely related to the dialects under study should not score significantly differently in a dialect intelligibility survey from others who are less bilingual or entirely monolingual. The critical factor is the careful screening of individuals participating in the testing such that no persons who have had intense contact with the non–hometown dialects being tested participate in the test.

Secondly, it is not clear that the formulation of sentence–tests would be significantly limited by the use of the principle of translatability. Morpho–syntactic structures such as those which I mentioned in connection with the Yawa intelligibility tests are the basic building blocks of language all over the world. While every single SL structure is unlikely to find a unique obvious counterpart in a given RL, we may certainly expect to find a substantial percentage of correspondences of structure which are easily and unambiguously translatable in both directions when these structures occur in their normal, unmarked functions. In addition, it should be noted that any limitation imposed by the principle of translatability on the formulation of sentence–tests nevertheless allows for a greater variety of grammatical structures and vocabulary to appear in each test (along with an increase in comparability) than is allowed by the limitations of the standard text–test approach discussed above.

Finally this type of test, like the standard text–test approach, requires a knowledge of the RL that can only be gained through a relatively long period of time studying one of the dialects to be surveyed. More on this below.

3. Conclusion

The sentence-based method of intelligibility testing outlined here is still in its embryonic stage of development, in that is has been field tested only in the Yawa language area of Irian Jaya. If this method is to become a genuine candidate to replace or supplement the standard text-test method in certain well-defined, multi-dialect situations, it must first be tested over a wider range of language situations to discover the limits of its usefulness. Further the sentence-test method needs to be applied to some of the same dialect situations to which the standard text-test method has been applied in order to compare the intelligibility percentages calculated on the basis of the two approaches. Use of this sentence-test approach to intelligibility testing in appropriate situations may not yield significantly different percentages of intelligibility from the standard text-test approach. I expect that it will at least exhibit an increased efficiency at obtaining those results and possibly narrow their inherent margins of error.

Of critical importance to this sentence-test method of intelligibility testing is that the investigator knows one of the dialects being surveyed, so that grammatically and lexically relevant, translatable test sentences can be devised. I have come to the conclusion that in the area of dialect surveying, as in other areas of linguistics, quick research is all too often inaccurate research. Sociologists regularly spend large amounts of time and care not only in the gathering and analysis of data, but in the design and testing of their survey instruments. Anthropologists invest enough time in a given cultural context to capture generalizations about that culture based on long-term observation. Linguists do the same thing in syntax, morphology, and phonology. Thorough analyses based on sufficient data take time. I would like to conclude that the same principle applies to dialectology. Clear, accurate analyses of multiple dialect situations anywhere in the world will only result when investigators invest sufficient time in background research to design relevant survey instruments which are adapted to the grammatical and lexical structures of the dialects to be surveyed.

NOTES

[1] I wish to express my appreciation to Joe and Barbara Grimes and to Gene Casad for their helpful criticisms of an earlier draft of this paper. I have tried to answer their objections to my satisfaction, if not always to theirs.

[2] It should noted, however, that this sentence-test method does not REQUIRE group testing as opposed to individual testing. It is adaptable to either method. The critical factor in using any intelligibility test with a group is the careful screening of the individuals in that group to ensure that none of them have had intense contact with the non-hometown dialects being tested.

[3] Of course, any method of intelligibility testing will be difficult, if not impossible, in areas where all speakers of a dialect are completely monolingual.

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APPENDIX

Examples of Scoring Elicitation Sentences for Intelligibility Testing:

1.	The child wants to open the door.	5 components
2.	Give me the other cloth!	5 components
3.	I walked to Ambaidiru, so my knees are sore.	6 components
4.	Whose child picked the papaya?	5 components
5.	If he drops the plate, I will be angry.	6 components
6.	Why is the child sleeping?	4 components
7.	I split the log with an axe.	5 components
8.	My father is the chief.	4 components
9.	Mother said she wants to come.	6 components
10.	My dog did not bite the child.	6 components ■

COMPUTING IN LINGUISTICS

STUDYING MORPHOPHONEMIC ALTERNATION IN ANNOTATED TEXT: Part One

by Gary F. Simons

A corpus of texts annotated in multiple dimensions can provide a wealth of material for the formulation and testing of linguistic hypotheses. The most recent installment of this column (Simons 1987) illustrated the range of information that can be stored in richly annotated texts, and described the *IT* package of software (Simons and Versaw 1988) which assists the linguist in producing such an interlinear text corpus. In this installment we look at one way in which the material stored in annotated texts can be mined to yield the wealth of information that is there. In this case, the particular application is the study of morphophonemic alternation. The method is presented in a series of ten numbered steps. Steps 1 through 5 appear below. Steps 6 through 10 will appear in the next issue.

1. Figure 1 gives a sample of the text corpus on which our example is based. If you have the next issue available, look ahead to figures 14 and 15 in part two to see the kind of morphophonemic information we will be deriving from the corpus. The language of the example is Northwest Alaskan Eskimo, also known as Inupiaq (Kaplan 1981). The texts were collected and analyzed by Wolf Seiler of SIL. The annotated texts used in this example are the first draft, before the insights gained from the kind of analysis illustrated here allowed the investigator to refine his analysis of the morphophonemic patterns. In the transcriptions, special characters which are meant to print out by means of a custom daisy wheel are represented by some of the ASCII special symbols; for instance, % represents the velar nasal and $ represents the palatal nasal. Hyphen is used in the practical orthography to separate a postclitic from the word to which it attaches. Note that this is indicated by equals sign (=) in the annotations.

Figure 1. Sample analyzed Eskimo text

```
\ref alco u 1

\tx Uqaaqtuuraallanialgitchua
\at uqaaqtu      -uraa     -llania     -lgit -chua
\mr uqaaqtuq     -uraaq    -llak-niaq  -lgit -tua
\mg telling story-gradually-DUR -INTENT-again-1s.IND
\wg I am going to tell a story again

\tx taivrumia                    qaa-im$a
\at ta  -ivru-mia                qaa-im$a
\mr ta  -im  -mia                qaa-im
\mg PROM-DA.D-DA.SG.MOD          when-DA.D
\wg about that one of long ago some time ago
```

The baseline text (\tx) is annotated in four ways: by allomorphic transcription (\at), by morphemic representation (\mr), by morpheme gloss (\mg), and by word gloss (\wg). Figure 2 gives the interlinear text model (ITM file) used to produce these annotations with *IT*. The two fields of interest for this study are the allomorphic transcription and the morphemic representation. In the allomorphic transcription, the analyst inserts morpheme breaks into the transcribed word as it appears in the base line. The spelling of the word does not change; only the breaks are added. The units thus delimited are the surface variants, or allomorphs, of the underlying morphemes. In the morphemic

representation, these allomorphs are mapped onto their underlying morphemes. This unification of surface variants to a single morpheme serves as the basis for the morpheme glossing. It also provides us with a database for investigating the whys and wherefores of morphophonemic alternation. (See pages 2-20 to 2-29 of Simons and Versaw 1988 for a full discussion of allomorphic transcription and morphemic representation.)

Figure 2. Interlinear text model for sample text

```
\reference_field marker ref
                 refers_to id

\text_field      marker tx
                 punctuation ";:()!?.,

\aligning_field  marker at
                 from tx
                 mapping_name Allomorphic_trans
                 itl_file eskimo.itl
                 morpheme_break --

\aligning_field  marker mr
                 from at
                 mapping_name Morphemic_repr
                 itl_file eskimo.itl
                 morpheme_break --

\aligning_field  marker mg
                 from mr
                 mapping_name Morpheme_gloss
                 itl_file eskimo.itl
                 morpheme_break --

\aligning_field  marker wg
                 from tx
                 mapping_name Word_gloss
                 itl_file eskimo.itl
```

2. For a study of morphology, whether of morphophonology or of morphotactics, the full text corpus is not really the proper database. We need only one instance of each word, whereas the text corpus is full of multiple instances of the words, thereby presenting us with a mass of redundant data. We want to base our study on a list of the words derived from the text corpus. For this we look to the lexical mappings database (ITL file) in which *IT* stores all the unique annotations. In the model of figure 2 we see that ESKIMO.ITL contains a mapping called *Allomorphic_trans* which stores the mapping from baseline words to their allomorphic transcriptions. *Lex-extr* is the program we use to extract this information into a standard format file that we can work with. We will name the extracted file WORDLIST.SFL. Figure 3 shows the beginning of that file. Figure 4 shows the *lex-extr* control file that was used to do the job. (Users of *IT* version 1.0 must add *from* and *to* parameters to the *\convert* field.)

Figure 3. Standard format dump of allomorphic transcription mapping

```
\type glossary
\atmc no
\sepr ;

\mppg name Allomorphic_trans from tx to at

\tx siqummirrutmik
\at siqummir-rut-mik
\auto

\tx akunnaqtuq
\at akun-naq-tuq
\auto

\tx sikutiqaa
\at siku-tiq-a-a
\auto

\tx killaɬmun
\at killaɬ-mun
\auto

\tx akutchitˆaiqsut
\at akut-chi-tˆa-iq-sut
\auto
```

Figure 4. *Lex-extr* control file for standard format dump

```
\one_gloss_per_line no
\gloss_separator ;
\convert mapping_name Allomorphic_trans
         itl_file eskimo.itl
```

3. Words typically have a single morphological analysis, but this is not always the case. When a surface word form has more than one way of being broken into morphemes, the *itp* program allows the user to enter multiple allomorphic transcriptions and make a choice in context for each word token occurring in text. When that mapping is extracted to a standard format file, such words will be associated with more than one allomorphic transcription. The control file in figure 4 declares that multiple annotations shall be put on the same line (*\one_gloss_per_line no*) and be separated by semicolon. For the data processing procedures which follow, it is necessary that each word in a *\tx* field be associated with just one analysis. This can be ensured by loading WORDLIST.SFL into a text editor, and then searching for a semicolon. If none is found, the file is in the proper format. Otherwise, the record containing the semicolon needs to be split into two. Copy the *\tx* field containing the base word to start the second record. Add an *\at* field, inserting what follows the semicolon as its contents. Delete the semicolon and what follows it from the first record. Repeat this procedure until there are no more semicolons.

Many of the fields in the SFL file are extraneous to our purpose. They may be left in the file and ignored, or they may be removed to make the file easier to use. The latter option is the one followed here (as can be seen in looking ahead to figure 7). The first four fields shown in figure 3 are unnecessary; so are the *\auto* fields. These may be deleted using your favorite text editor. Another way to remove them is with the field

exclusion feature of the TA (JAARS 1987) program. In fact, if you are using TA you can sort the word records into a user-defined alphabetical order at the same time.

4. WORDLIST.SFL now contains every unique mapping of word to allomorphic transcription in the corpus. But it does not contain the data we need to study morphophonemic alternation, namely, the relationship between surface forms and underlying lexical forms. The mapping from individual allomorphs to their underlying morphemes is saved in the *Morphemic_repr* mapping, but that is insufficient for our purposes. We need to map the allomorphic transcription of the whole word to the morphemic representation of the whole word, so that we can study the allomorphic variants in context and thereby explain what is going on. We will get those word-level morphemic representations the same way we got them in the first place—by running *itp* to annotate the allomorphic transcriptions. Since we have already done this on the texts, all the allomorphs and their underlying forms are already entered into the lexical database. This annotation step will thus proceed almost completely automatically.

Before we can run *itp* on WORDLIST.SFL we must convert it to ITX format. This is the job of *itprep*. As we look at WORDLIST.SFL (figure 3), we see that it is nearly in ITX format already. It has \tx as a baseline and \at as an aligning annotation. The key missing ingredient is the reference field for each unit of the database. We want each word with its annotations to be a unit. This means we can use the \tx marker as the item in the input file which uniquely identifies the beginning of each unit. We declare this in an *itprep* control file as illustrated in figure 5. Note that we also increase the output line length to 60; the default is 40, but we might run into some words longer than that.

Figure 5. *Itprep* control file for changing SFL dump to an ITX

```
\# Convert SFL dump of \tx-\at mapping to ITX file

\unit_level_markers tx
\output_line_length 60
```

Itprep needs another input, namely, the text model (ITM file) for the ITX file it is to produce. A sample is given in figure 6.

Figure 6. Interlinear text model for annotated word list

```
\reference_field marker ref

\text_field      marker wd
                 punctuation ",;:()!?.,

\#\aligning_field marker at
\#                 from wd
\#                 mapping_name Allomorphic_trans
\#                 itl_file eskimo.itl
\#                 morpheme_break -=

\#\aligning_field marker mr
\#                 from at
\#                 mapping_name Morphemic_repr
\#                 itl_file eskimo.itl
\#                 morpheme_break -=
```

There are two things to note in the model file of figure 6. First, the text field marker is declared as *\wd* (for "word"). This label is a bit more meaningful since these fields contain words rather than lines of text. The real reason, however, is that version 1.0 of *itprep* gets confused when the unit-level marker in the control file and the text field marker in the model file are the same thing. Second, notice that the aligning field declarations in the model have *\#* in front of them. This turns them into comments ignored by *itprep*. If *itprep* finds fields in the input file which are declared as aligning annotations in the model, it will not copy those lines into the output. This is to avoid the possibility that the contents of those fields are not properly aligned and thus that the output might not be a valid ITX file. We avoid this conflict by commenting out the *\at* field declaration for the time being. The result of running *itprep* on WORDLIST.SFL with the control file of figure 5 and the model of figure 6 is shown in figure 7. This file is called WORDLIST.ITX.

Figure 7. Word list as ITX file

```
\itm figure6.itm

\ref tx 1

\wd siqummirrutmik
\at siqummir-rut-mik

\ref tx 2

\wd akunnaqtuq
\at akun-naq-tuq

\ref tx 3

\wd sikutiqaa
\at siku-tiq-a-a

\ref tx 4

\wd killa#mun
\at killa#-mun

\ref tx 5

\wd akutchit^aiqsut
\at akut-chi-t^a-iq-sut
```

5. WORDLIST.ITX is now ready to run through *itp* in order to add the morphemic representation analysis. Before this can be done, the model file shown in figure 6 must be edited to remove the comment markers (*\#*) blocking the definition of the aligning annotations. The annotated WORDLIST.ITX produced as a result of running *itp* with the full model is illustrated in figure 8. Note, that one may add as many other annotations as desired for other purposes. For instance, to develop the database for analysis of morphotactics, one could add annotations for morpheme categories and subcategories. Illustrated here are only the annotations needed for the current application.

Figure 8. Annotated word list

```
\itm figure6.itm

\ref tx 1

\wd  siqummirrutmik
\at  siqummir-rut-mik
\mr  siqummit-uti-mik

\ref tx 2

\wd  akunnaqtuq
\at  akun -naq-tuq
\mr  akuti-naq-tuq

\ref tx 3

\wd  sikutiqaa
\at  siku-tiq-a -a
\mr  siku-tiq-aq-a

\ref tx 4

\wd  killaɬmun
\at  killaɬ-mun
\mr  killaq-mun

\ref tx 5

\wd  akutchitˆaiqsut
\at  akut  -chi-tˆa-iq-sut
\mr  akutuq-si -tla-iq-tut
```

We have now constructed a database of analyzed words from the lexical mappings database of our text corpus. We are ready to commence the series of data transformations that will convert our wordlist into a database of morphophonemic alternations. This will be taken up when the article continues in the next issue of this journal.

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A BRIEF NOTE REGARDING DISCOURSE-CENTERED PH.D. STUDIES AT THE UNIVERSITY OF TEXAS AT ARLINGTON

Robert E. Longacre

I do not think we have to argue very vigorously any more to convince most linguists that something on the nature of a 'discourse revolution' has taken place in linguistic studies both in the United States and abroad. Increasing dissatisfaction with the study of sentences in isolation has led to a variety of developments. Some university programs have simply attempted to add on a further module called Discourse Studies, Pragmatics, or something of that sort on top of a pretty much unchanged syntax-centered curriculum. Others are beginning to see that the full impact of discourse studies is such that it is bound to affect the various levels of linguistic analysis so that it will prove impossible to isolate neatly the discourse work from other aspects of linguistic study.

I think a very interesting example in this regard is the current program which is being proposed at University of California at Santa Barbara with a strong faculty in discourse including Wallace Chafe, Sandra Thompson, Charles Li, and others. They are pushing for a graduate program which will be discourse-oriented but will do everything that linguists have always done and will probably continue to do such as phonology, morphology, syntax, etc. I'm taking one very interesting sentence from page 7 of their proposal: 'We are intrigued by the possibility that the current primacy of syntax (in the form of the study of the structure of isolated sentences) as the keystone of linguistic structure will ultimately yield to encroachments from discourse-centered explanations on the one hand, and to morphological considerations on the other.'

It has long seemed to me and to some others within the SIL that a linguistics which is frankly discourse-centered (but does not fail to do what linguists have always had to do), precisely fits the needs of people engaged in Bible translation work and in linguistic analysis preparatory to Bible translation work. The practical question arises, therefore, of the selection of a graduate school for studies on the M.A., and especially, on the Ph.D. level in accordance with such an emphasis. In this brief notice, I'm especially concerned with doctoral studies on the Ph.D. level and possible programs which give proper place to discourse studies. For some years, our program here at UTA was one of the few (if not the only) place in the country where one could get Ph.D. work in linguistics with a strong emphasis on discourse. In recent years, our program at UTA has become more structured than it used to be. Although it always had been a humanities degree with concentration in linguistics, in recent years our program has been structured with a number of core courses, which, of course, occupy a significant portion of the students' time and proportionately weaken the concentration in linguistics. Possibly as a result of this tightening up of the program, or perhaps on account of some other as yet unanalyzed factors, we no longer seem to be getting SIL people as Ph.D. candidates at the University of Texas at Arlington.

I would like to suggest in this notice that the prospective doctoral student within the Summer Institute of Linguistics should take a second look at our Arlington program before he decides that he wants to opt for a program in which he can get a Ph.D. in linguistics rather than one in humanities with concentration in linguistics. To explain this, I want to use the metaphor of a three-storied house (one that I've used in print a place or two). The bottom story of the house of discourse studies embodies a simple insistence that every ordinary working linguist (OWL) needs to have a *discourse orientation* whether he is in Phonology or Morphology or Syntax Lexicography or what have you, i.e. he/she needs to have a least peripheral vision for discourse and to understand its possible relevance at every stage. The second floor of discourse studies is one on which linguists frankly engaged in the study of the form and meaning of

texts. *Text linguistics*, as a specialized discipline within linguistics should be especially concerned with the interface between the morphosyntax and the discourse structure. Everything a person has learned by linguistic analysis on the lower levels is examined and questioned in regard to its import for communication and for the structure of discourses, conversation or monologue. Here questions of prominence and foregrounding, cohesion, coherence, and many matters of this sort come in for careful consideration by the discourse specialist. There is, however, still a third floor to the house of discourse studies. This is the result of the fact that one does not go very far in discourse studies before one comes up to an interface between linguistics and other disciplines, e.g., sociology, psychology, artificial intelligence, literary studies, and even more specialized fields such as history, jurisprudence, and theology. For this reason, invariably discourse studies on the part of many people cannot terminate within the field of linguistics proper but must be enlarged beyond the field of linguistics into a discipline which can be called *Text Theory*.

What does all of this have to do with the selection of a graduate school on the part of an SIL person looking forward to graduate studies in linguistics? Much. To begin with, there are not many schools where discourse studies constitute a significant and pivotal part of the program. The University of Oregon is such a place, and now the University of California at Santa Barbara is getting such a program going. I heartily recommend these programs to SIL colleagues. What of the graduate program at UTA? A proper appraisal of our programs lies with realizing that a humanities program with concentration in linguistics is ideally constituted for those who want to inhabit the third-floor of the house of discourse studies, i.e. Text Theory. At this point the Humanities core requirements, instead of being a nuisance become a virtue. They give a quite necessary orientation to relevant matters in philosophy, hermeneutics, rhetoric and literary theory. At the same time the program is flexible enough that deep and serious research in discourse or text linguistics is possible. And all this is in turn backed up by a competent linguistics faculty with varied strengths: in contemporary and more traditional phonological theory, in morphology, in syntax (whether of the more formal variety or one more centered on language as communication), historical linguistics, sociolinguistics, and certain area studies.

Discourse studies are represented especially by Ilah Fleming, Shin-ja Hwang, and myself — although others on the linguistic faculty are making increasing contributions in this regard and the Translation Department offers several significant courses for the student of discourse. Occasional courses are offered from time to time by such obscure people as Kenneth L. Pike and Ken Gregerson — whom we are more that glad to have teach a course whenever we can cajole them into doing it.

I suppose that I am somewhat in the role of the old man of linguistic discourse studies by now. I was early in the field compared to most of those now working in discourse, and my early tools were not always the most well adapted to the job. The tools have since been sharpened by coming into contact with European text linguistics (especially van Dijk), by my supervision of discourse studies in a variety of languages from around the world, by experience gained in overseas workshops, and by my now rather extensive research into the discourse structure of Biblical Hebrew. Some people, I'm afraid, still regard me primarily as the author of *Grammar Discovery Procedures* (1964). From my present stance, however, I have to regard the 1964 volume as a piece of sentence-grammar and as benighted as any work which is thus limited.

I have approximately five years more (at the most) to teach at UTA before I retire and change assignment. I am presently directing three dissertations, in each of which I am keenly interested. Others are coming on. None are by SIL people. Let me put it frankly: I would like to direct a few more dissertations by SIL people before they bring the curtain down on my career as a full-time university professor! ■

CV-ANALYSIS

Ursula Weisemann

Prologue

This is the first in a series of articles which contain questionnaires. They are designed to help you research the language you are involved in by giving you pointers on what you need to know in order to research the relevant questions. Today we know a lot about the various parts of phonology and grammar of many languages. I propose to put at your disposal what I've learned in Africa on the background of my work in South America and subsequently taught to a number of native speakers of African and other languages. For this, I'll try to outline the questions at hand. Sometimes I can present the most complicated system I've encountered of the particular feature in question as well as the simplest one. I'll try also to summarize the most important functions of the structures encountered.

I would be most interested in your reactions as to:

1) the validity of the proposed questions and their explanation in your research situation;
2) what you think about the validity of the samples I will show;
3) whether what you have is significantly different from what I propose, and how.

I hope that you've seen the pronoun volume which I edited: 1986 *Pronominal Systems* (with an introduction by J.H. Greenburg), Tübingen: Gunter Narr Verlag. It contains papers, mainly by SIL authors, on the pronominal system of 49 non-European languages. Most of them were done working from a questionnaire which is included in the volume.

Unfortunately, the book is a bit expensive. But since it is selling quite well the publishing house is interested in similar volumes on other topics. Maybe we can do one each on the different questions I'll present to you (with the exception of vowels and consonants, maybe). So if you'd like to write a paper on any of the subjects, feel free to send it to me for inclusion in such a volume. I'll correspond with you on questions arising out of the paper, doing consulting by mail. I will send you formatting specification for your paper and suppose that you'll be doing it on the computer. If you want to publish your paper elsewhere, feel free to do so (don't forget to mention where you got your ideas from), but I'd still be interested in a copy. Please let me know when you send your paper (which may be in prepublication form) what you want done with it.

The best place to send it might be:

Ursula Wiesemann
S.I.L., B.P. 06-2552
Cotonou, Benin

With a copy to me at:

Ursula Wiesemann
Postfach 603, D-5909
Burbach-Holzhausen, Germany

Thank you! I am looking forward to hearing from you.

CV-Analysis

The following are suggestions for analyzing vowels and consonants using the intuition of the native speaker, your language assistant. These will work only if your language assistant is literate in another language. This can be a European or any other language. But the basic letters should be the same. That is, if you're going to use the Roman alphabet, he should be familiar with it, though his language might need some further symbols not normally found in the 24 letter alphabet. If he knows syllabic writing, obviously what I'll be saying here cannot apply the same way. It is based on using the Roman alphabet with the addition of further (hopefully nationally acceptable) letters for certain sounds.

The analysis of the phonological system of your language can be speeded (and indeed enhanced) if you learn to draw on the intuition of your language assistant. The basis for alphabet and orthography rules has to be psycholinguistic, i.e. you should symbolize what the native speaker recognizes. Short of peering into his head, here are some techniques that help you do it. In Africa we use this method to teach 'mother tongue linguists' how to research their own language. I have not found a way of doing it with someone who does not know how to write, however, and even less so with someone with whom I cannot communicate in some language. So you can only do it with an assistant who already has some book learning, and a language that you and he have in common.

First give the assistant a quick introduction into articulary phonetics: points and manner of articulation and some phonetic symbols. Actually we work with the symbols the country has adopted for possible orthographies and show them the approximate phonetic value. It has to be approximate because we don't want to encumber him with phonetic details. If this introduction was not enough to give him the symbols for all the sounds he is conscious of in his language, listen to the sound that bothers him and give him another symbol.

The next step is to ask him to write word lists, oriented to word initial consonants. He should have a minimum of 10 words on each list and use a new sheet for each consonant. All the word initial consonants should be covered (illustrated) by a list. Then also, on the same sheet, the same consonants should be explored within the word and at the end of the word. If you have consonant clusters you need to explore these with lists as well. And the same work needs to be done for the vowels — a separate list for each vowel wherever it occurs in consonant initial and vowel initial words (though don't bother about which consonant precedes it at this stage).

The next step is to listen to the lists. Have the assistant pronounce each list separately for you. You listen to observe if the sound, which the list was made for, stays the same in all the words on the list or if there are variant pronunciations that he has written the same. If there are variants, try to determine (for yourself) the conditioning factors. If you are satisfied that you have found the rule, keep it in mind to be sure it always works. If not, try questioning the assistant if he indeed hears the sound the same in all the words or if he has mixed two sounds in the same list. Don't be emphatic though, work with him by questioning rather than by informing for fear of skewing his intuition. Keep in mind though that, particularly in the beginning, his intuition is likely to be obscured by the language in which he has learned to read and write.

As you listen to the words on the lists you give him some subtle training in what is a syllable in his language. You do this by selecting a clear CVCV type word and ask him how many syllables this word contains. Do this several times. Then move onto more difficult patterns, slowly moving up to the ones you are in doubt about. It is a good idea to be in doubt about most any pattern that contains anything but CV

sequences, i.e. what looks like CCV, or CVV, or anything else. This training is best repeated over several days until the assistant has slept as least once over it. Once his responses sound confident and stay the same, you can start believing the data you get. It might be good, though, in all of this analysis, and particularly in the matter of syllable division, to work with more than one person to double check.

You might wonder why the syllable division is so important. I've found that the matter of interpretation is the most sensitive part of the phonological analysis of any language, and in order to know how to interpret you have to know what is one or more than one syllable. If you can train your assistant to tell you, you're way ahead. And you can't tell from the pronunciation. Take an example: [pra] would be one syllable in Ewe (Togo) but two in Mandara (Cameroon), and as far as I am concerned, I can't hear the difference!

Once you know what constitutes a syllable, you know which instances of [i,u] are to be interpreted as /y,w/ (when they are not the only vowel in the syllable). Remember that here might also be a [i]:/w̆/ correspondence. As far as I can see, in Africa you are likely to find short and long (i.e. identical but lengthened) vowels in one syllable but no 'diphthongs.' Long vowels often might best be interpreted as /Vh/ depending on the other possible syllable patterns in your language. That is, the vowel length might be a manifestation of a syllable coda, most likely an /h/.

Having figured out which consonants and vowels you have and what kind of syllable types, you are ready to make the distribution chart. I've found that to be the most valuable tool in CV analysis, but only if it is as complete as possible.

To make the distribution chart you need big sheets of lined paper. Put the consonants in a column at the left, the vowels in a row at the top. In the resulting cells, fill in one word containing the CV syllable that is indicated by the CV combination. Put in the whole word and its translation. Your assistant can do this for you.

You need to make as many charts of this kind as you have syllable types. If you have palatalization and/or labialization, you need to try every consonant for this feature, combined with every possible vowel. It may be necessary to do that with every possible CC combination as well. If you also have V.V combinations across syllable boundaries, you need to chart these. As for syllable final consonants, you need to check only on the ...VC combinations. If you have C.C combinations across syllable boundaries, you also need to chart these.

The data contained in this chart should give you all the data necessary to interpret the phonemes. Watch for holes in any of the many charts you will have before you and try to fill them in by oddities from the same or another chart. In other words, try to reduce your system as much as possible. In Africa we find that vowels particularly are easily colored by the labialization and palatalization processes or features, and the distribution chart will bring this out by showing up the defective distribution of such vowels.

There is no shortcut for this distribution chart. Besides exploiting his knowledge of the language, your assistant in filling it out, will learn much. In my consulting, I refuse to pass an opinion on a CV system of any language unless I have all the evidence before me on such a chart. Then I double check myself by having the assistant pronounce for me any trouble spots. This is easy because the data is neatly laid out before you. I often ask him to pronounce the word by giving him the gloss in the language we share so he doesn't see what he has written. Sometimes the assistant will try to introduce differences that aren't really there, particularly in the vowels. So I use my ears to double check his work . But I also rely on him: if he gives me minimal pairs that I

have a hard time distinguishing, but he insists that there is a sound difference, I accept that and try to improve my hearing. If, however, you work on a tone language, he might be tricked into putting his attention in the wrong place! Particularly since at this point he has not yet worked with the tone (see separate suggestions on how to work with the tone).

Having in this way worked through the whole system, you should be ready to make your phonological write up. If at all possible co-author it with your assistant. ■

☞ WANTED: MORE PITHY SAYINGS ☜

We are still in great need of pithy sayings — this time to adorn the inside walls of three buildings, instead of only one. A meaningful and intellectually stimulating saying will enhance the minority-language peoples in the eyes of visitors to Dallas' SIL. The International Linguistics Coordinator is desperate for proverbs, riddles, poems, analogies, or other succinct expressions put in the idiom with a translation.

Please send your suggestions in the form of your regular orthography with both a phonetic transcription (for the benefit of pronunciation by our tour guides) and a translation. For example:

Ñan ko ñɪ cil
(ɲan ko ɲɪ čɪ)
Bitterness kills its owner
- Lamba proverb, Togo -

Why not gain a little recognition for the group you work with? All pithy sayings should be addressed to:

Eugene Loos
Linguistics Coordinator
7500 W. Camp Wisdom Road
Dallas, TX 75236 USA ■

THE 14TH INTERNATIONAL SYSTEMICS WORKSHOP

Reported by Sherri Brainard

Systemics refers to systemic functional grammar which is a linguistic theory developed by Michael Halliday. Halliday is known for his work on transitivity and theme. (He created the theme/rheme distinction.) He is also known for the book *Cohesion in English* which he coauthored with his wife, Ruqaiya Hasan.

Systemic linguists are interested in language in its context. This interest was demonstrated in two studies of casual conversation.

The first study was conducted by Dorothy Economou. Economou teaches English to Greeks who have immigrated to Australia. She was interested in teaching her students how to engage in appropriate casual conversation within an urban Australian setting. She noted that it was essential to be aware of the context in which people engage in casual conversation when the participants are strangers. An understanding of the context was necessary if a model is to be provided for the students to imitate in order to learn how to have casual conversations in another language in another culture.

The first step in analyzing the context of casual conversation is to decide what the majority of the members of a society share as common knowledge when they do not know each other well. Within the urban Australian setting, topics of common knowledge are work, leisure, health, home and family life. She noted that when strangers engage in casual conversation, they do not explore differences; instead, they explore similarities.

In an effort to develop an actual model for her students, Economou decided to concentrate on movies since movies served as a topic of casual conversation in both Australia and Greece. What she found out, however, is that people in Greece discuss different things about movies than do people in Australia. The reason for this is that movies have a different function in Greek culture compared to Australian Culture. In Greece, movies are cheap, and there are continuous showings. People go to movies all the time. In Australia, movies are expensive, and they are arranged social events that often include eating out at a restaurant as well. While people in Greece talked about the characters, plot and specific scenes of the movie, people in Australia talked about the events of the whole evening rather than the actual story of the movie.

Although Economou did not achieve her goal of designing a teachable model of casual conversation, she did gain important insights into how the context of a speech situation affects the speech event.

The second study concerning casual conversation was conducted by Di Slade and Susan Eggins, graduate students in linguistics at the University of Sydney. Their research question was: What is casual conversation and can it be taught for language learning? Other questions raised by their investigation were:

1. How is casual conversation structured?
2. How does it start?
3. How does it keep going? What motivates it?

Slade and Eggins discussed some of the findings of their investigation as follows:

First, casual conversation between strangers is different from casual conversation between friends. As had been noted, strangers explore similarities while friends explore differences.

Second, casual conversation is different in structure from other discourse genre because it can sustain itself. It strings different types of discourse genre together, such as factual narratives, jokes, gossip.

Third, agreement and opinion are central to the structure of casual conversation. A minimal text in casual conversation is defined as an opinion followed by an agreeing reaction. If the hearer disagrees with the opinion, then the exchange possibilities alter. For example, what began as a casual conversation could change into an argument. If the hearer expresses a neutral, noncommittal reaction, the reaction functions as a stalling device and implies eventual disagreement. If the hearer disagrees, then he must supply evidence and resolution in order for the casual conversation to continue. Otherwise the conversation is likely to terminate. In fact there is danger that the conversation will terminate anyway if the evidence and resolution are delivered aggressively.

Fourth, the element that appears to motivate and sustain casual conversation is interpersonal relations. There seems to be some sort of cooperation principle which keeps casual conversation from disintegrating into an argument which would, in turn, sever interpersonal relations. One of the ways in which the cooperation principle works is that some of the members of the conversation perform a 'rescuing' function to keep the exchange going so that the interpersonal relations are kept intact. This 'rescuing' function includes tactfully defending someone when they are being cornered in a discussion, making a joke to diffuse the tension of the discussion, and changing the topic of the conversation to one that is more neutral. The whole point of the casual conversation is to keep it going because it reinforces interpersonal relations.

I found these discussions of casual conversation intriguing and had visions of sitting on the bench at one of the sari-sari stores in the village (or any location that serves as a common meeting place) and taping casual conversations. It seems to me that even a cursory study of casual conversation in the village would have benefits in language and cultural learning. Some questions that might produce useful insights are:

1. What do people in the village talk to each other about?
2. What do people in the village talk to strangers about?
3. What is the implied information in the conversation?
4. What cultural values are expressed in the opinions?
5. How do we engage in casual conversation with people in the village? (There are probably differences in regard to us because while we are not strangers we are not true insiders.)

For those who would like to read about systemic linguistics, the following two books are short, well-written introductions. They contain good definitions of terms commonly used by systemic linguists.

Halliday, M.A.K. and Ruqaiya Hasan. 1987. *Language, context, and text: aspects of language in a social-semiotic perspective.* Victoria: Deakin University Press.

Hasan, Ruqaiya. 1985. *Linguistics, language, and verbal art.* Victoria: Deakin University Press.

The following volume,

> Hasan, Ruqaiya, ed. 1985. *Discourse on discourse.* Wollongong, N.S.W.: Applied Linguistics Association of Australia.

is a collection of four papers produced during a systemics workshop on discourse. This volume includes discussions of casual conversation, classroom discourse, expository discourse, and narrative discourse. The paper on expository discourse is one of the few available on the topic and is highly recommended to anyone working with expository texts. ■

SPECIAL ISSUE ON CODE MIXING AND CODE CHOICE

☞ CALL FOR PAPERS ☜

Carol M. Eastman of the University of Washington will be editing a special issue of the *Journal of Multilingual and Multicultural Development*, which will be published in 1992. This issue will be devoted to codeswitching and codemixing in a cross-cultural context — with an emphasis on the process as it occurs in urban (language-contact) areas around the world. Papers dealing with the options available to language planners in urban context that take into consideration mixing-switching phenomena would be appropriate. Similarly papers addressing the social, psychological and cultural factors governing switching and/or mixing would be suitable. The role of code switching/mixing in pidginization and creolization is also of interest. It is anticipated that the papers collected in the volume will share a focus on language use as an interactive process shaping the reality in which it occurs as well as shaped by it.

Please send your suggestions and contributions in sufficient time to:

Professor Carol Eastman
Department of Anthropology
University of Washington
Seattle, Washington 98195 USA ■

THE FIFTH INTERNATIONAL CONFERENCE ON AUSTRONESIAN LINGUISTICS

Reported by Julie King

1. Conference Structure and General Impressions.

During the four-and-a-half day conference, more than 60 papers were presented which should indicate that the schedule was quite full. There were four slots per day where papers were presented, and three papers were presented during the hour-and-a-half slots. During two of those time periods there were papers being presented in two different rooms and so there was that amount of choice as to which section one attended.

Several well-known Austronesianists were in attendance at the conference and most of them presented papers. Among them were Dyen, Blust, Reid, Starosta, Laycock and Bruce Biggs. I was disappointed that neither Professor Wurm nor Shopen from ANU were in attendance.

Since my return to Malaysia, and even while still in Auckland, I tried to come up with some generalizations about the papers presented or to note some trends in the field of Austronesian linguistics, but had little success. I found it rather frustrating that the *Big Boys* of Austronesian are arguing proto-Oceanic and proto-Austronesian phonemes while the lesser known scholars are working in a vast hodge-podge of interest areas. There were papers on anything from stylistics to 'An Eskimo substratum in East Polynesian'. (I have to admit that I ducked out on the latter.) There were only a couple of sections on Western Austronesian, with a disproportionate number of papers covering a wide variety of topics on Pacific languages. That is, of course, understandable considering the venue of the conference.

2. SIL participation in VICAL.

I felt that SIL was well represented at the conference with several very good papers and no bad ones. The quality of the SIL participants was also very high and I felt we were all well-received and taken seriously. We had good interaction together and with other participants.

3. Attending Conferences and Presenting Papers.

Some who read this report will remember a similar section in my report of TICAL in 1981. In that report I gave my impressions of the conference and cautioned about taking the matter of presenting papers at an international conference lightly. I am not taking back any of what I said in that widely circulated report. I will concede however that since the entire posture of this conference was much more 'laid back', participants were more accepting and less condemning of rather poor papers and performances. I guess I felt that I will be quicker to be willing to present papers in the future because of my experience at VICAL but I still maintain that we need to present our very best side at such conferences. At this conference our members were very professional and presented well-documented and well-researched papers.

4. Contacts made.

I was glad to have contact with people from ANU. I was also glad to meet and talk with Stanley Starosta of Hawaii and have had more contact since the conference. He has sent me some literature on his Lexicase theory which I will certainly be making available to the library (after I read it all myself). I also had good contact again with Lawrence Reid (also of Hawaii) and he has informed us that even though we are committed to archiving at Oxford, we can still archive with them at Hawaii as well. We will have to make committee decision about that as the logistics of archiving at all are rather difficult to get worked out and even more difficult to implement. ■

REPORT ON VICAL

The Fifth International Conference on Austronesian Linguistics held at the University of Auckland January 11–16, 1988.

Barbara Dix Grimes and Charles E. Grimes

THE PAPERS

PAN: Many of the well known Austronesianists were present, including Isidore Dyen, Robert Blust, Andrew Pawley, Lawrence Reid, and Stanley Starosta . Their papers and current work continues to be at least partly focused on 'top–down' arguments regarding the subdivisions of Austronesian with particular focus on Formosa and Melanesia at this conference. Dyen in his paper co–authored with Shigeru Tsuchida (Tokyo Univ.), 'Proto–Philippine as the closest relative of proto–Formosan' suggested a high level chain of Formosa–Philippines–West Indonesia.

The most recent stir in Austronesian circles is Malcolm Ross' (ANU) 'Proto–Oceanic and the Austronesian Languages of Western Melanesia' which will appear as *Pacific Linguistics* C–98. At the conference many acknowledged this as the most significant work to appear recently on Austronesian languages and that it has forced a reconsideration of many aspects of Austronesian classification.

Oceania: Polynesian and other Oceanic papers were well represented with several on stylistics of Maori and Hawaiian. Margaret Mutu Grigg's (Univ. of New Zealand) paper was a good historical overview of 'Theoretical approaches to Polynesian grammatical description' and could serve as a useful role–model for a helpful review of the literature and theoretical issues related to an area.

Language Contact: Contact between Austronesian and non–Austronesian (Papuan) languages continues to be of interest. Current approaches include looking at pre–European contact in the Pacific which could have influenced the development of Melanesian Pidgin, the cover term now used to include the separate 'varieties' of Tok Pisin, Bislama (Vanuatu), and Solomon's Pijin.

Indonesian: Marit Kana (Wm. Carey Univ.) gave a good presentation of 'Bahasa Indonesia Adversatives as Clause Unions'. She did a good job of explaining relevant aspects of relational grammar to her audience. John Little (in Sumatra with the Univ. of Kentucky project) gave a well laid out presentation of 'Indonesian Malay verbs that have *built–in* directional meaning.'

Sulawesi: Jim Sneddon's (Griffith Univ.) paper on 'The position of Lolak' is, as he himself describes it, a classic example of how lexical similarity led him to initially make a wrong classification of Lolak. Based on lexical similarity he had grouped Lolak with Mongondow, but after doing proper reconstruction found to his surprise that Lolak is a Gorontalic language with heavy borrowing from Mongondow, a prestige language. There is a lesson in this for all of us involved in SIL surveys. Lexical similarity is not the final chapter on language classification.

Orthography: An orthography paper on the old problem of /w/ and /y/ in a West New Britain Province, PNG language was given by R. Goulden (McMaster Univ.). Also, D. Arms (Missionary Society of St. Columban) on orthography problems in Fijian. Comments by several people showed that orthography is an area where some Austronesian linguists have a felt need for help. Those who are working on dictionaries or government sponsored projects realize orthography issues are not always easily settled.

Dictionaries: One of the exciting things at VICAL was the presence of a Raga speaker from Vanuatu, Richard Leona, along with his wife. He is the co–producer with David Walsh (Univ. of Sydney) of the Raga dictionary. His short paper 'Dictionary making

in the South Pacific: A Raga viewpoint' stimulated a lot of discussion about the methods and politics of dictionary making. He concludes:

> Why do we Raga people want our language to be made into a Dictionary? The reasons are as follows:
>
> (1) Because many words in our language are starting to be lost by the younger people.
> (2) Our language is a very important part of our culture — if we lose our language it means that we lose much of our culture.
> (3) When God made the world He put us on the surface of the world, then He gave us our languages — if we lose our language we lose our gift.
> (4) Today in Vanuatu Raga children are taught at school in English or in French .... Also, another language of Vanuatu is Bislama, the national language, which Raga children use a lot and which is not their mother tongue. They need to have a dictionary for Raga, their mother tongue, to help them to learn about their own customs and culture.
> (5) The Raga Dictionary will make other Ni-Vanuatu thing about making dictionaries for their own languages, and it may help to show them how it is done.

Phonology: Autosegmental phonology has made its way to the Austronesian world with Juliet Levin's (Univ. of Texas at Austin) presentation on 'Mokilese autosegmental phonology: The interaction of boundary lengthening and assimilation.' Ian Maddieson (UCLA) gave a very complete description of linguo–labials (tongue tip/blade to upper lip) found in several Vanuatu languages.

OTHER COMMENTS

An observation we will pass on for future reference relates to the reception of different kinds of papers at VICAL and earlier ICAL conferences. The first, and most common type of paper is a *data paper*, either descriptive, comparative, or historical. Data papers are well received if the writers have done their homework with both the data and the literature, and address the issues involved. They are dealt with quite harshly if they have not done their homework, and this is true of the old–timers as well as the newcomers. A *synthesis paper* has almost a 100% failure rate by those new to the field. It is best to be thoroughly saturated with both the literature and the data (on a wide range) over a long period of time (ten years?) before making bold pronouncements about areal features. The third type are the *theory–bound papers* (bound tightly to a particular theoretical framework to the point that the audience has to be expert in that framework to follow the paper, distinguished from data papers which address theoretical issues and which are welcomed). These are received negatively across the board. Some have been able to save the day by carefully and patiently bridging the gaps of terminology and notation for those not steeped in the particular model, but that is clearly the exception. The Austronesian gang has little patience for papers that are so restricted in notation and theoretical assumptions of a certain model that one must be a specialist to follow it. Similarly, they have little patience for papers that use highly selective data to substantiate a point of particular theoretical model, rather than using a theoretical model to illuminate the data. The fourth type of paper we will call *attempts at a snow job*. These are papers that are written for the sake of writing a papers and playing the academic game. Sometimes they are done by choosing a topic that is deliberately so restrictive that the probability of any one person following a major portion of it is minimized. These papers tend to come across as an avoidance of contributing to our understanding, rather than an attempt at contributing to our understanding. They don't fool anyone and are a waste of everyone's time and money.

Indonesia and Malaysia are great gaping holes in our knowledge of Austronesian. Several areas in which SIL can be making significant contributions are: top–quality surveys of little known areas; solid morphological and syntactic description; topic continuity in discourse; comparative studies (of high caliber); and even things like hairy phonological analysis as it gives insight to orthographic solutions. ■

SECOND LANGUAGE ORAL PROFICIENCY EVALUATION

PLEASE PRINT -- COMPLETE ALL INFORMATION

NAME *(Last, First Middle)*			___OK TO RECORD ___DO *NOT* RECORD
AGENCY			
SOCIAL SECURITY	DATE OF BIRTH *(Month/Day/Year)*	SEX: M F	SIGNATURE

LANGUAGE	CODE	TESTING PLACE	
TEST DATE *(Month/Day/Year)*	DAY OF WEEK	TIME	TESTER
TEST RESULT		LINGUIST	

RATING SCALE

	BLOCKING	*DYSFUNC-TIONAL*	*INTRUSIVE*	*ACCEPTABLE*	*SUCCESSFUL*	*SUPERIOR*	*RAW SCORE*
COMPRE-HENSION	Misunder-standings of very simple language. 0-4	Examinee recognizes non-under-standings which are quite frequent. 5-9	Difficulties in compre-hension slow down the interaction. 10-14	Exhibits understanding quickly when clarification, repetition or paraphrase is given. 15-19	Understanding is limited by cultural background. 20-24	Understanding is essentially complete. 25	
DIS-COURSE	Unable to produce continuous discourse. 0-4	Minimally elaborate performance. 5-9	Performance shows lack of cohesion. 10-14	Discourse is cohesive. 15-19	Discourse is well-organized. 20-24	Broad, flexible control of functional rhetorical devices. 25	
STRUC-TURE	Structural accuracy is largely random. 0-5	Miscommuni-cation arises frequently due to lack of control of structure. 6-11	Inconsistent accuracy in relatively high freqency areas. 12-17	Structural inaccuracy is rarely the major cause of misunder-standing. 18-23	Range and control of the structure is great. 24-29	Structural accuracy and flexibility approach bilingual competence. 30	
LEXICAL-IZATION	Lexicalization is inaccurate and narrow. 0-5	Lexicalization range is narrow and highly unnatural. 6-11	Lexicalization is unusual and/or imprecise. 12-17	Lexicalization is clear and relatively natural. 18-23	Very infrequent imprecision of lexicalization. 24-29	Lexicalization is precise. 30	
FLUENCY	Examinee speaks with difficulty. 0	Fluency is irregular. 1-3	Examinee speaks readily but not fluently. 4-5	Examinee speaks readily, suitably filling in pauses. 6-8	Performance is fluent. 9	The flow of speech is effortless and smooth. 10	

TOPICS: Warm Up_____

Getting Information_____ RAW SCORE TOTAL : _____

Giving Information_____

RAW SCORE:	0-5	6-18	19-31	32-44	45-57	58-70	71-83	84-96	97-109	110-118	120
S - SCORE:	0	0+	1	1+	2	2+	3	3+	4	4+	5

SIGNATURES:_____ _____

LINGUIST TESTER

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UPCOMING EVENTS

The *IIIrd International Euralex Congress, Budapest* will be held from **September 4th through September 9th, 1988.** Papers are invited on all aspects of lexicography, theoretical and practical, diachronic and synchronic. The main fields of interest: general, computational, terminological and specialized translation, lexicography. Deadline: November 15, 1987. For further information write to the Congress Organizer, Ms Judit Zigány, Akadémiai Kiadó, 1363 Budapest, P.O. Box 24, Hungary.

The *Linguistics Association of Great Britain and British Association for Applied Linguistics* will meet **September 14–16, 1988** at the U. Exeter, Exeter, England. Contact: M.W. Wheeler, School of Modern Languages, U. Of Liverpool, P.O. Box 147, Liverpool L69 3BX, England.

The *5th Eastern States Conference on Linguistics*, ESCOL V, is to be held in Philadelphia at the University of Pennsylvania from **Friday, September 30 through Sunday, October 2, 1988.** Papers are invited on any topic in linguistics and the deadline is June 10, 1988. For more information contact Dr. Jack Hoeksema, Dept. of Linguistics, Univ. of Penn., Philadelphia, PA 19104–6305.

The *Thirteenth Annual Meeting of the Semiotic Society of America* will be held October 27–30, 1988 at the University of Cincinnati. Abstracts were due by April 10, 1988. This year the theme is 'Sign as Reality, Sign as Illusion,' although papers on other topics in the field of Semiotics are also invited. Contact Dr. Gila Safran-Naveh, Chair of Local Arrangements Committee, College of Arts & Sciences, ML #169 McMicken Hall, 50C, University of Cincinnati, Cincinnati, OH 45221. Phone: (513)321–3070 or 475–2297.

The *30th Annual Meeting of the Midwest Modern Language Association* will be held from **November 3–5, 1988** in St. Louis, Missouri. Proposals for papers to the Linguistics I Section should be devoted to any aspect of anaphora or cataphora in English or other languages. The Nominating Committee hopes to include papers dealing with anaphoric phenomena from a variety of perspectives, including syntactic, semantic, discourse, pragmatic, psycholinguistic, comparative, historical, and sociolinguistic treatments. The deadline was April 11, 1988 for proposals. Please contact the Section Chair: Ellen Barton, Linguistics Program, Department of English, Wayne State University, Detroit, Michigan 48202. Notification is the beginning of May, and papers must be completed by August 18.

The *Symposium on Language Universals* (organized under the auspices of the Belgian Linguistics Circle), will be held at the University of Antwerp, from **December 9–10, 1988.** All papers on universalist research will be considered. The deadline is June 1, 1988. Send 4 copies of a 1-page abstract to: Johan van der Auwera, U. of Antwerp, Linguistics (GER), Universiteitsplein, 1, B-2610 Wilrijk, Belgium.

The *Fourth Congress of the International Association for Semiotic Studies* will be held at Barcelona from **March 30 to April 1, 1989** and at Perpignan from **April 2–6, 1989.** The theme will be 'Humanity and its Signs' with one of the following perspectives: Semiosis in communication, Semiosis in culture and society, and Semiosis in the human sciences. The deadline was March 15, 1988 for proposals. Contact the Secrétariat du IV° Congrès de l'AIS, IRSCE, Chemin de la Passio Vella, Université de Perpignan, 66025 PERPIGNAN CEDEX for further information. For travel and accommodation contact Catalogne Voyages, 59 avenue du Gènèral de Gaulle, 66000 PERPIGNAN. ■

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NOTES FOR CONTRIBUTORS ■ Readers are invited to submit letters of comment and/or publishable materials to the editor of *NL* at **The International Linguistics Center, 7500 W. Camp Wisdom Road, Dallas, TX 75236**.

MANUSCRIPT STYLE ☐ Intending contributors should ensure that their manuscripts conform to the standards laid down in the LSA Style Sheet printed periodically in the *LSA Bulletin*.

COMPUTER MEDIA ☐ Contributors who have copies of their manuscripts on magnetic media such as 5¼" disks in PC/MS-DOS or CP/M format, or on TU58 DEC tapes are urged to submit the same along with their manuscript. [5¼" PC/MS-DOS disks are preferred.]

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Coordinator's Corner

EUGENE LOOS

This issue we begin a new format of NL. We have moved from the awkwardness of a daisy-wheel printer to an easy-to-handle dot matrix, to a laser. Our first issues with a laser were done in 10 point type reduced 70%. The theory was that if we used this point size and reduced it, the 300 dots per inch resolution of the printer would in effect produce the equivalent of 450 dots per inch, but the reduction process eroded the characters giving us less legibility rather than more. Readers found the tiny print difficult. We hope that the unreduced 10 pt type of this issue will be more attractive; it is simpler for us to produce.

Lou Hohulin's review presents clearly the heart of Lakoff's controversial thrust in relating (or rejecting) theory and linguistic argumentation. Lou's article prompts me to mention that we have received a number of new books for review. Please indicate if you would like to do a review for us. It is an excellent way to refresh yourself with new thinking. Here are titles we have available for review:

Aczel, Peter. *NON-WELL-FOUNDED SETS*. 127 pp.

Baker, Mark C. *INCORPORATION, A theory of grammatical function changing*. 543 pp.

Barlow, Michael and Charles A. Ferguson, eds. *AGREEMENT IN NATURAL LANGUAGE, Approaches, theories, descriptions*. 355 pp.

Buck, Carl Darling. *A DICTIONARY OF SELECTED SYNONYMS IN THE PRINCIPAL INDO-EUROPEAN LANGUAGES*. 1515 pp. of fine print, 8.5"x11" page size.

Gazdar, Gerald, and Alex Franz, Karen Osborne, Roger Evans. *NATURAL LANGUAGE PROCESSING IN THE 1980s*. 236 pp.

Hilbert, David. *COLOR AND COLOR PERCEPTION, A study in anthropocentric realism*. 146 pp.

Johnson, Mark. *THE BODY IN THE MIND, The bodily basis of meaning, imagination, and reason*. 233 pp.

Newmeyer, Frederick J. *THE POLITICS OF LINGUISTICS*. 171 pp.

O'Grady, William. *PRINCIPLES OF GRAMMAR AND LEARNING*. 233 pp.

Pereira, Fernando C.N. and Stuart M. Sheiber. *PROLOG AND NATURAL-LANGUAGE ANALYSIS*. 266. pp.

Pollard, Carl and Ivan A. Sag. *INFORMATION-BASED SYNTAX AND SEMANTICS, vol 1 Fundamentals*. 233 pp.

Staal, Frits. *UNIVERSALS, Studies in Indian Logic and Linguistics*. 267 pp.

Uszkoreit, Hans. *WORD ORDER AND CONSTITUENT STRUCTURE IN GERMAN*. 190 pp. ■

4

Roots and Concerns of Typological/Functional Linguistics

THOMAS E. PAYNE

University of Oregon & Summer Institute of Linguistics

1. Historical Background

The distinctly American approach to the study of language stems from descriptive work on indigenous languages conducted by Franz Boas in the late 19th and early 20th centuries. Boas introduced the notion that each language must be understood on its own terms, not necessarily subject to rules and constraints devised in the context of the classical languages. Under the influence of Boas and his followers, notably Sapir, grammatical descriptions were largely restricted to phonological, morphophonemic and morphological statements, rarely extending beyond structural features of words. Nevertheless, a collection of texts and a dictionary nearly always accompanied the publication of a descriptive grammar. The reason for this practice was that language was viewed as a product of culture. The linguistic system, though not itself necessarily describable in universal terms, was of interest only as it fit into an integrated analysis of the whole of culture. Hence texts, i.e. written artifacts of human discourse, were the optimal linguistic examples. The dictionaries were valuable storehouses of cultural and contextual information, necessary to an insightful interpretation of the texts. To this day, these sets of grammar, dictionary and texts form some of the richest material available on many North American languages.

In the period of prevailing empiricism between 1930 and 1960, the relativistic views of Boas led to an American structuralism in linguistics that attempted to divorce the description of the structure of language from the meaning. The reasoning behind this movement was that since we cannot assume any universals of 'abstract structures' (such as meaning), we can only describe the forms. This movement had the positive effect of instilling a scientific rigor and concern for methodology into the study of language, but also caused a decline in the perception of language as a product of culture and a concommitant decline in the value of descriptions of little-known languages.

The post-structuralist work of American linguistic theorists, informed by rationalism, began to concentrate on construing abstract structures on the basis of observed data. This work generally assumed that, though meaning may have structure in and of itself, it is logically autonomous of the abstract and concrete formal structures of sentences (i.e. syntax). Although under this view linguistic investigation was extended to the sentence level, it did not go beyond that. Discourse structure was assumed to be non-existent or at least too loose to be described with scientific rigor — a matter of 'preference' or 'performance' rather than scientifically predictable behavior. Hence questions of the influence of intentionality, pragmatics, style, culture and history on language were considered to be outside the domain of linguistic description, and the compilation of texts and dictionaries continued to decline as a priority for most linguists.

"... the functions of language in a community will have describable, and even predictable, effects on linguistic form."

Throughout the period of American structuralism and post-structuralism several American linguists, notably Pike and Bolinger with input from Europeans such as the Prague school (Mathesius 1929) and Halliday, kept alive the empirical traditions of Boas and Sapir while adding an authentic concern with discourse and human communication. These linguists accepted the definition of linguistics as a science, but embraced the fact that language always exists within a community of speakers. One of the tenets of this approach is that the functions of language in a community will have describable, and even predictable, effects on linguistic form. The return of this approach to the mainstream of American linguistics can be traced to Greenberg (1963, 1978, *inter alia*).

The work of Greenberg represented a return to the Boasian emphasis on describing structures on their own terms, without necessary appeal to universal abstract principles. The difference was that Greenberg observed that there were such things as SUBSTANTIVE UNIVERSALS (as opposed to 'formal', or abstract universals). When many languages are compared simply on the basis of their surface structures, it is found that they are not arbitrarily different from one another, neither are they arbitrarily complex. Rather, they fall into certain definable types with recurring characteristics independent of their genetic relationships.

Greenberg's work represented a broadening of the goals of linguistics to include empirical investigation of large numbers of genetically diverse languages. However, substantive universals were at first mere observations of statistical regularities among samples of languages, and for several years did not transcend the sentence level. More recently, Greenberg's remarkable substantive universals have given rise to a renewed interest in explaining linguistic phenomena in terms of the whole human experience. It was recognized almost immediately that many substantive universals simply could not be satisfactorily explained in terms of paradigms that omitted even such basic notions as meaning from the set of possible conditioning factors, much less pragmatics, style, poetics, culture, etc. (see, e.g., García 1984, Givón 1979, 1983, 1984).

". . . linguistics . . . is poised to begin to incorporate explanations from such domains as style, rhetoric and metaphor into the substance of linguistic description."

This relatively recent movement towards a more wholistic view of language has come to be known as TYPOLOGICAL/FUNCTIONAL LINGUISTICS. The thrust of this enterprise is first to typologize, i.e. determine the range of actual variation observed in the corpus of documented natural languages available, then to attempt to *explain* that variation, including deviations from the norm, in terms of established functional principles. Such principles are drawn from semantics, pragmatics, and other domains outside the formal linguistic system itself. From this point of view an explanation or definition of a structural phenomenon in terms of structure itself is insidiously circular. Hence the principal ambition of this approach is the discovery and elaboration of 'functional explanations'. Finally, the formal and functional analysis of speech styles has attracted considerable research interest within a subfield of linguistic anthropology called the *ethnography of communication* (Hymes 1977).

Other research in this subfield includes ethnopoetics, ceremonial dialogue, language and music, rhetorical structure and discourse analysis.

In summary, we see a steady progression within linguistics from the analysis of phonemes and morphemes, to words, then sentences, and finally to discourse. To stretch an analogy, even as morphology is the

study of morphemes within words, syntax the study of words within sentences and discourse analysis the study of sentences within discourse, so the ethnography of communication is the study of discourse within its social and cultural setting. At this point in its historical development, linguistics, at least within the typological/functional tradition, is poised to begin to incorporate explanations from such domains as style, rhetoric and metaphor into the substance of linguistic description. Current work in discourse pragmatics (Givón 1983, 1984), comparative speech styles (Sherzer 1977, Urban 1985), and metaphor (Lakoff and Johnson 1980), is evidence that this integration is already beginning to take place.

2. Distinctives of Typological/Functional linguistics

For the functionalist, the discovery of substantive grammatical universals is an invitation to explanation from outside the linguistic system. For some linguistically–coded distinctions, e.g. AGENT versus PATIENT, the functional motivation is obvious; in this case the motivation is that speakers universally need to communicate *who is acting upon whom*. It is less obvious, hence of some interest, why languages would nearly universally differentiate what appear at first blush to be purely structural categories such as subject and object. For a pure structuralist, grammatical relations are either primitives, or are defined in terms of other structural primes. As such they are not themselves subject to definition from outside the realm of syntax.

A basic example of the application of functional principles to the explanation of Greenberg's universals concerns the notion of SUBJECT. Greenberg suggested that subject is a category which is practically always identifiable in surface structure terms in languages of the world. Where it is identifiable, the subject nominal nearly always precedes other nominal elements in basic clauses. This simple observation has led to much discussion of the definition and functional nature of the subject category, and recently to a rethinking of fundamental notions such as 'basic clause', 'grammatical relation' and 'pragmatic role'. In this case the functional motivation for the near universal syntactic properties of subjects turns out to be multidimensional, i.e. the notion of subject represents a conflation of the empirically testable functional notions 'highly topical' and 'highly agentive', operative within human limitations of memory and attention.

A major distinctive of the typological/functional approach to language is reliance on the notion of prototype in linguistic categorization. A prototype is the 'best example' of an element of a particular category, e.g. a sparrow is probably very close to the prototype for the category *bird*. A category based on a prototype is constituted by a set of items that bear a 'family resemblance' to the prototype. The exact nature of that resemblance is something that has stumped linguistic and cognitive scientists for decades. This is because human beings are able to make connections from one item to another (e.g. to say *this* is an instance of *that*) based on past experience, native perceptual and cognitive capacities, and situational context, that no formal model can duplicate (Rosch 1976, 1978, Lakoff and Johnson 1980, *inter alia*). Metaphor is perhaps the clearest example of this capacity to construe resemblances. To understand a sentence like *she is a rose* one must make a connection between the referent of *she* and a concept of *rose* that is based on far more than what we would consider a dictionary definition of *rose*. Certainly we do not mean she is a member of the genus *rosa*, or that she has thorns and soft petals. In fact, despite the formal similarity between the sentences *she is a rose* and *it is a rose* (uttered while pointing to a particular flower), these two sentences really convey very different ideas. Although the surface structure does not code the difference, English speakers have no trouble understanding which sense of *rose* is intended in each sentence. This is true even for newly coined metaphors, e.g. I could imagine telling a fellow student that the test was *a real lion* and he would immediately understand something about my perception of the test, even though I presume he had never heard that particular metaphor.

"... understanding the reason ... requires the creative capacity of the human mind. Structuralist and post-structuralist linguists are uncomfortable with this principle ..."

Similarly, structural categories such as subject may be instantiated by items with vastly different relational properties — in the same way as the term *rose* can refer to vastly different items in the world, including a class of flowers and certain people. To say that subject is a purely structural category is like saying *rose* is a purely structural category, with no semantic content whatsoever. As such, the definition of *rose* would simply be the set of all instances of things that can be referred to with that label, without regard to why this particular set of items

and not some other. The fact of categorization is self-evident; understanding the reason for a particular clumping of elements within a category requires the creative capacity of the human mind.

Structuralist and post-structuralist linguists are uncomfortable with this principle because of the 'fuzziness' of the notion 'family resemblance'. It is very difficult, if not impossible, to construct a formal theoretical model that incorporates this notion. Nevertheless, To belabor the category of subject, the fact that English categorizes the grammatical relation borne by *John* in *John built a house*, *John was burned*, *John fell* and *John underwent surgery* as **the same** (as manifested by linear order, verb agreement, etc.) is phenomenal. Formal theories typically develop elaborate methods of dealing with this kind of formal categorization, ranging from ignoring the possiblility of a functional basis for the category to positing abstract 'levels' of formal structures. The typological/functional approach says that there is a prototypical element of the category 'subject'. On the basis of this prototype, nominals which 'resemble' the prototype are treated as members of the category.

3. Current Concerns in Typological/Functional Linguistics

3.1. Grammatical and lexical description

In order to engage in typological research, theoreticians must first have at their disposal accurate and detailed grammatical descriptions of a wide range of human languages. To date there are still many areas of the world where reliable and usable grammatical descriptions are lacking. Within the typological/functional tradition a mode of grammatical description has developed that is free of inordinate dependence on formal devices (diagrams, systems of annotation, abstract structures, etc.). Instead the emphasis is on prose description, supported by many examples, preferably drawn from language in use. Depending on the needs of the individual researchers, the resulting descriptions may be brief sketches, detailed technical analyses, pedagogical grammars or traditional reference grammars. In most, if not all, cases lexical descriptions consist of bilingual dictionaries. Much current typological/functional work involves conscious attempts to refine the art of linguistic description in the direction of clarity of presentation, with a solid understanding of the centrality of the cultural setting of language.

". . . the demand for closely glossed and analyzed discourse data is increasing dramatically."

Quite apart from the value of grammatical and lexical description for adding grist to the mill of functional theorists, such description has value in its own right. For example, an entire subfield of linguistic anthropology, termed ETHNOSEMANTICS, deals with lexico–semantic categorization as an ethnographic tool. One specific example of how the study of grammatical categories sheds light on larger questions is the use of *evidential* marking in native American languages. Chafe and Nichols (1985) have termed evidentiality the 'linguistic coding of epistemology'. Many languages contain in their verb paradigms categories which reflect the reliability or source of the information encoded in the verb. For example, in Panare (a Carib language of Venezuela) each verb has two past participles. One participle is used if the speaker personally witnessed the state described by the verb, while the other is used if the speaker only heard about it from someone else. Other languages have much more complex evidential systems, encoding several degrees of reliability.

Studies undertaken without a function–based approach to linguistic description have often misinterpreted such evidential distinctions; usually they have been classified as 'aspects', or 'tenses' following the Indo–European model. Recently, systematic investigation of the evidential systems of a variety of languages, has yielded significant insights into native perceptions of knowledge. Of course, good linguists working within any paradigm are able to determine the functions of linguistic categories such as evidentiality simply out of intense interaction with the data. However, formal theories themselves do not provide the tools for understanding such categorization. Functional studies such as Chafe and Nichols (1985) are attempts to come to grips with functional principles and their formal instantiations in a wide variety of languages. Such studies do provide frameworks for any linguist who finds him or herself up against data for which a formal theory has nothing to say. (See also Greenberg 1978, Shopen 1985 for other function–based frameworks for the description of morphosyntactic categories).

3.2 Discourse analysis

"A central item on the agenda of contemporary functional linguistics is to determine . . . how those principles are instantiated in discourse . . ."

The analysis of discourse is basic to the study of language, literature and culture. First, without a thorough understanding of the culture–specific ways in which people structure their thoughts in discourse, an outsider is often bewildered by apparently random collocations of ideas. Second, documentation of discourse data without an understanding of the rhetorical and stylistic principles at work results in a superficial and esthetically dry rendering. Finally, by observing other ways of structuring discourse, we become conscious of the narrowness of our own modes of thinking.

Within typological/functional studies researchers are typically concerned with the following issues in discourse analysis:

Development of a corpus. A corpus of discourse, appropriately collected, transcribed and annotated, is indispensable as a source for the study of literary style, as well as of various facets of culture from the native point of view. The quality of the conclusions an investigator can reach in such studies is directly related to the size and quality of the corpus on which they are based. In preparing material in a form that will be transparent to others, researchers must understand the rhetorical principles and their formal instantiations at work in the corpus. Hence developing a corpus requires dedicated analytic work.

Within linguistics the demand for closely glossed and analyzed discourse data is increasing dramatically. This is due mostly to new emphases on the importance of discourse–pragmatic and socio–cultural factors to morphosyntactic form and on data from a wide range of language areas and linguistic types. As evidence for this assertion we note that at least two articles in the December, 1987, issue of *Language*, the journal of the Linguistic Society of America, present discourse–based analyses of Amerindian languages (Du Bois 1987, D. Payne 1987). The latter article, in particular, points out the importance of cultural knowledge in understanding the choice between one grammatical form and another.

Rhetorical structure. One set of 'esthetic resources' (Sapir 1921:228) specific to any language consists of devices used to impose rhetorical

(sometimes 'thematic') structure upon the content of discourse. At least since Sapir, linguists have recognized that discourse is hierarchically structured according to rhetorical principles. A central item on the agenda of contemporary functional linguistics is to determine the rhetorical principles (the functions) and how those principles are instantiated in discourse (the forms).

Narrative discourse, for example, can be structured according to the principles of *time*, *place* or *logic*. Individuals engaged in narrative tend to appropriate one of these principles and downplay the common connective that links high level discourse units in narrative is a locational expression translatable as *from there*. In English, and many other languages, connectives such as *and then* (time), or *and so* (logic) more commonly serve this linking function. This, combined with many other facts of Yagua grammar and discourse style, confirms that locational orientation is a fundamental discourse structuring principle for Yagua speakers — more so than for speakers of many other languages (T. Payne 1985).

Much of the work of Grimes (1975), Longacre (1976, 1985) and many others centers on the use of grammatical particles, semantic parallelism, emphatic operators and other devices in communicating the rhetorical structure of discourse. More recent work, e.g. Mann and Thompson (1985), T. Payne (1985) and Fox (1986) has attempted, with some success, to come to grips with the rhetorical analysis of discourse structure using empirically testable definitions and techniques. A major thrust of typological/functional studies is to elucidate the principles by which speakers of each language tend to organize their thoughts in discourse, and the formal indications of those principles. In accord with this emphasis, attempts are being made to extend work in rhetorical analysis beyond narrative, e.g. to descriptive, hortatory and procedural discourse.

Discourse pragmatics. Recent interest within functional linguistics centers on the influence of discourse–pragmatic factors in speakers' choice among various grammatical structures in discourse. One specific area of recent debate is the question of constituent ordering. The inherited wisdom of American and European linguistics contends that 'thematic', 'presupposed', 'given' or 'old' information precedes 'rhematic', 'asserted', or 'new' information in sentence structure (Mathesius 1929, among others). This generalization, forwarded by speakers of Indo–European languages on the basis of introspective data concerning their own native tongues, was raised to the status of

a near axiom. More recently it has been shown for many non–Indo–European languages (Tomlin and Rhodes 1979 for Ojibwa, T. Givón 1983 for Ute, Derbyshire 1985 for Hixkaryana, D. Payne 1987 for Papago, and others) that in actual discourse 'new' or 'asserted' information tends to precede 'given' or 'presupposed' information.

3.3 Ethnography of communication

A recent sub–discipline within the field of linguistic anthropology has been termed the ETHNOGRAPHY OF COMMUNICATION (Hymes 1977). Researchers in this areas study indigenous patterns of language use, comparative speech styles (Sherzer 1977, Urban 1985), the use of metaphor (Sapir and Crocker 1977), multilingualism (Sorensen 1967), language death and language attitudes. Specific research goals include: 1) to discover and define the genres and speech styles, including prose, poetry and measured verse where applicable, of the language, 2) to determine the social functions of each genre or style, 3) to understand the influence and nature of written versus oral literature in native communities, and 4) to point out and interpret the use of metaphor, direct quotation, rhetorical questions, rhetorical negation, etc., in the material being studied.

4. Conclusion

The major goal of typological/functional linguistics is to expand the notion of FUNCTIONAL EXPLANATION for linguistic phenomena in the direction of a more inclusive understanding of the role of language within the human experience. This goal is to be achieved principally through intense interaction with a corpus of contextualized linguistic data. Therefore, the documentation of discourse data as well as grammatical and lexical descriptions of a wide range of languages in a form that is understandable to many researchers is central. Other important tasks include the articulation and development of theories of functional principles, including semantics, discourse pragmatics, rhetorical structure, style and poetics.

"... expand the notion of FUNCTIONAL EXPLANATION for linguistic phenomena."

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On the Renarrative Mood in Bulgarian

ALEKSANDER B. MILCH

0.0. Introduction

The present paper is continuation of the series of articles initiated with ' The Bulgarian Participle ' (*Notes on Linguistics*, 35). As stated in that first article, the series is intended to continue the work of the late Professor Charles E. Bidwell, i.e., to provide English-speaking linguists and beginning slavists with concise structural descriptions of aspects of Slavic languages. Fortunately, since the late 1960's, when most of Bidwell's work was done, the great paucity of linguistic information on th e Slavic languages has been relieved somewhat, and hence the goal of this series is to shed light specifically on the more rare aspects of Slavic languages for which there are no counterparts in the Western languages, and for which there is still a dearth of information. While Bidwell emphasized structural description in order to delineate inflectional morphology (i.e., to describe a morphophonemic aspect of language), in this paper I expand the method to include syntax as well as morphophonemics.

1.0. Semantic Overview

The Renarrative Mood is also known as the Indirect or Distanced Mood,[1] and Bulgarian, as *Preizkàzno Naklonénie*, or simply *Preizkàzvane*. This mood is employed in order to relate an event or situation that was not actually witnessed by the speaker, but learned about through a third party. (It is quite different from the phenomenon of Reported or Indirect Speech, which is used merely to restate someone else's words.) Since the Renarrative Mood is used to communicate essentially 'second-hand' information, its very use on the part of a speaker will imply doubt, surprise, irony, or merely an emphasis of the fact that it is someone else's interpretation of an event and that the speaker disclaims responsibility for the veracity of what is being said. Alternatively, if the speaker does not feel the need or purposely does not want to relate the fact that what he is saying has been learned through a third party, he is not obliged to use the Renarrative Mood. The Renarrative Mood is customary in folklore, legends, and historical writings. The importance of the speaker's feelings toward

"The only other Slavic language to have Renarrative Mood is Macedonian. . ."

what he is relating, which is intrinsic in the choice of whether or not to use the Renarrative Mood, is reminiscent of the choice involved in using the Subjunctive Mood in Romance languages. The only other Slavic language to have a Renarrative Mood is Macedonian, in which it is called *Prekažanost*.[2]

2.0. Formation

2.1. The Present Renarrated

The Present Renarrated corresponds to both the Present Indicative and the Imperfect Past Tense. It is formed with the appropriate Present Tense form of *sŭm* 'be', and the Imperfect Participle of the main verb.[3] In the Present Renarrated, as well as in all other renarrated forms, the third-person singular and plural forms of *sŭm* (*e* 'is' and *sa* 'are') are deleted.

tòj znàe 'he knows'	>	***tòj znàel*** 'they say/I hear that he knows'
tòj znàeše 'he used to know'	>	***tòj znàel*** 'they say/I hear that he used to know'
vìe dòite 'you milk'	>	***vìe ste dòeli*** 'they say/I hear that you milk'
vìe dòexte 'you used to milk'	>	***vìe ste dòeli*** 'they say/I hear that you used to milk'
àz glèdam 'I look'	>	***àz sŭm glèdal*** 'they say/I hear that I look'[4]
àz glèdax 'I used to look'	>	***àz sŭm glèdal*** 'they say/I hear that I used to look'

2.2. The Aorist Renarrated

The Aorist Renarrated corresponds only to the Aorist Indicative. The Aorist Renarrated is formed with the appropriate Present Tense form of *sŭm* and the Aorist Participle of the main verb.

nìe čùxme 'we heard'	>	***nìe sme čùli*** 'they say/I hear that we heard'
tì mìslì 'you thought'	>	***tì si mìslìl*** 'they say/I hear that you thought'
tjà valjà 'she rolled'	>	***tjà valjàl*** 'they way/I hear that she rolled'

2.3. The Perfect Renarrated

The Perfect Renarrated corresponds to both the Present Perfect and the Past Perfect. It is formed with the appropriate Aorist Renarrated form of *sŭm* and the Aorist Participle of the main verb.

tòj čèl e	>	***tòj bìl čèl***
'he has read'		'they say/I hear that he has read'
tòj bèše čèl	>	***tòj bìl čèl***
'he had read'		'they say/I hear that he had read'
àz poglèždaí sŭm	>	***àz sŭm bìl poglèždal***
'I have glanced'		'they say/I hear that I have glanced'
àz bjàx poglèždal	>	***àz sŭm bìl poglèždal***
'I had glanced'		'they say/I hear that I had glanced'
tè stojàli sa	>	***tè bíli stojàli***
'they have stood'		'they say/I hear that they have stood'
tè bjàxa stojàli	>	***tè bíli stojàli***
'they had stood'		'they say/I hear that they had stood'

2.4. The Future Renarrated

The Future Renarrated corresponds to the Future Indicative and the Past Conditional. It is formed with the appropriate form of the auxiliary verb *šta* in the Aorist Renarrated, the conjunction *da*, and the appropriate Present Indicative form of the main verb.

àz šte znàja	>	***àz sŭm štjàl da znàja***
'I will know'		'they say/I hear that I will know'
àz štjàx da znàja	>	***àz sŭm štjàl da znàja***
'I would have known'		'they say/I hear that I would have known'
àz šte dŭrzà	>	***àz sŭm štjàl da dŭrzà***
'I will hold'		'they say/I hear that I will hold'
àz štjàı da dŭrzà	>	***àz sŭm štjàl da dŭrzà***
'I would have been holding'		'they say/I hear that I would have been holding'
vìe ste strèljate	>	***vìe ste štèli da strèljate***
'you shall shoot'		'they say/I hear that you shall shoot'
vìe štjàxte da strèljate	>	***vìe ste štèli da strèljate***
'you would have been shooting'		'they way/I hear that you would have been shooting'

2.5. The Future Perfect Renarrated

Although the Future Perfect Renarrated is archaic in speech, and thus no longer a productive form, it is included here for the sake of completeness. It corresponds to the Future Perfect Indicative and the Past Future Perfect Indicative. It is formed with the appropriate Aorist Renarrated form of the auxiliary verb *šta*, the conjuction *da*, and the appropriate Aorist Renarrated form of the main verb. In the formation of the Futuer Perfect Renarrated, third-person forms of *sům* are only deleted in the first Aorist Renarrated construction, i.e., the Aorist Renarrated form of the auxiliary verb *šta*, but are retained in the Aorist Renarrated form of the main verb.

tì šte si tùrpjàl > ***tì si štjàl da si tùrpjàl***
'you will have suffered' 'they say/I hear that you will have suffered'

tì štja da si tùrpjàl > ***tì si štjàl da si tùrpjàl***
'you were going to have suffered' 'they say/I hear that you were going to have suffered'

tè šte sa čèli > ***tè štèli da sa čèli***
'they will have read' 'they say/I hear that they will have read'

tè štjàxa da sa čèli > ***tè štèli da sa čèli***
'they were going to have read' 'they say/I hear that they were going to have read'

vìe šte ste čùli > ***vìe ste štèli da ste čùli***
'you will have heard' 'they say/I hear that you will have heard'

vìe štjàxte da ste čùli > ***vìe ste štèli da ste čùli***
'you were going to have heard' 'they say/I hear that you were going to have heard'

2.6. Emphatic Renarration

Emphatic Renarration is the renarration of information that has already been renarrated.

Tòj bìl cetjàl knìga.
'I hear that they say that he is reading the book.'

Emphatic Renarration carries the same implications of skepticism, surprise, etc., as Renarration. Emphatic Renarrative forms can be formed simply by adding the Aorist Participle of *sům* to the normal Renarrative form. Emphatic forms, however, cannot be formed from normal Renarrative forms if the Renarrative form already contains the element *bil*. Hence, all of the Renarrated forms have counterpart Emphatic Renarrative forms, save the Perfect Renarrated.

Pres. Renar.: *tòj znàel*	>	**Emphat. Pres. Renar.:** *tòj bìl znàel*
Aor.Renar.: *nìe sme čùli*	>	**Emphat. Aor. Renar.:** *le bìŭ sme čùli*
Perf. Renar.: *àz sŭm bìl poglèždal*	>	**Emphat. Perf. Renar.:** —
Fut. Renar.: *vìe ste štèli da strèljate*	>	**Emphat. Fut. Renar.:** *vìe bìŭ ste štèli da strèljate*
Fut. Perf. Renar.: *tì si štjàl da si tŭrpjàl*	>	**Emphat. Fut. Perf. Renar.:** *tì bìl se štjàl da si tŭrpjàl*

2.7. Negated Renarrative Forms

In the Present, Aorist, and Perfect Negated Renarrated forms, the negative particle, *ne*, immediately precedes the entire verbal form.

tòj znàel	>	*tòj ne znàel*
nìe sme čùli	>	*nìe ne sme čùli*
àz bìl poglèždal	>	*àz ne bìl poglèždal*

(As always, the negative particle, *ne*, does not carry any stress.)

The negation of Future Renarrative forms is more complex. It is formed with the invariable negated Aorist Renarrated form of *sŭm—njàmalo*,[5] the conjunction *da*, and the appropriate Present Indicative form of the principal verb.

vìe ste štèli da strèljate > *vìe njàmalo da strèljate*

The negation of the Future Perfect Renarrated is formed with *njàmalo*, the conjunction *da*, and the Aorist Renarrated form of the main verb.

tì si štjàl da si tŭrpjal > *tì njàmalo da si tŭrpjàl*

Emphatic Renarrative forms are negated by inserting the negative particle, *ne*, immediately before the entire verbal form, i.e., before the chose form of *bil*.

tòj bìl znàel > *tòj ne bìl znàel*

NOTES

[1]The terms DISTANCED MOOD and INDIRECT MOOD are seldom used. The term INDIRECT MOOD is used predominantly in Hubenova's textbook (currently the standard textbook in Bulgarian), but is avoided in general for fear of confusion with

INDIRECT SPEECH, which, as is pointed out below, is quite another phenomenon. The term DISTANCED SPEECH/MOOD is rarely used in reference to Bulgarian's Renarrative Mood, but rather to that of Macedonian, especially in Lunt's textbook.

[2]For an explanation of the formation and use of Macedonian's Renarrative, or 'Distanced', Mood, see Horace Lunt's textbook, *Grammar of the Macedonian Literary Language*, pp.91–97.

[3]There is a controversy as to whether the Imperfect Participle is used only in the Renarrative Mood, or whether it is used in other verbal forms as well. According to deBray and Scatton, the former is the case, whereas Aronson supports the latter position (see his review in *Slavic and East European Journal* 30:1, pp. 132–134).

[4]When used in the first person, the Renarrative Mood will especially imply doubt or shock as to the veracity of what has been said, e.g., 'They say/I hear that I look at . . . — they're saying that about *me*!?', as if to imply that what is being said is 'news' to the speaker.

[5]The negation of ***sûm*** is ***njàma***, which is a contraction of ***ne*** and ***sûm***, and hence ***njàma*** already contains the element ***sûm***. Thus, in the Aorist Renarrated form of ***njàma***, it is not necessary to repeat the element ***sûm***; it is only necessary to provide the Aorist Participial form of ***njàma***, viz. ***njàmalo***.

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☞ *WANTED: ARTICLES ON DIALOGUE JOURNALS* ☜

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Niger-Congo Noun Class Systems

URSULA WIESEMANN

A number of the Niger-Congo languages have a noun class system. It commands the cross referencing of participants in a text. Most of the nouns belong to a class of singular forms and again to a class of plural forms; some have one form only without distinguishing singular and plural, often called neuter forms. Tone changing rules often are part of the system.

In Bantu studies it is customary to keep the singular and the plural classes separate, even though occasionally a singular class and a plural class may have the same markers. The combination of which singular classes combine with which plural classes makes the gender distinction within the system; the neuter classes are a gender each by themselves.

1. The markers

In order to research the whole system, a series of markers needs to be studied. Some of the markers might be the same for more than one class; on the other hand, several markers might occur with different nouns of the same class. Therefore the whole set of possible markers has to be examined before the system can be unfolded properly. This includes the following markers:

a) *Prefixes and/or suffixes on the nouns*. These can be discovered by researching the singular and the plural forms for each noun. It is best to work with lists at this point without trying to assign class membership, as there may be more than one affix form for the same class

b) *The subject prefixes on the verb* (the subject pronouns) are in concordance with the noun classes, so there will be a set of as many prefixes as there are classes. This is true for the different kinds of subject pronouns found in the language: non-emphatic, differing sets according to the mood of the verb, etc.

c) The same concordance holds for *other pronoun sets* the language might have: object, indirect object, possessive "adjectives" such as 'my head' or 'my house' and possessive "pronouns" such as 'mine'.

d) *The possessive marker* usually found in such constructions as 'the dog of the chief' contrasts with the genitive of 'the bone of the dog'. The possessive marker that can be translated 'of' may concord with the possessor or with the possession. This is where subtle tone changes might become indicators of class membership. There might be a similar construction in the language, differing from the preceding simply by absence of the 'of' morpheme, in such constructions as 'house of clay clay house'. In such constructions the concording morpheme is absent; if its presence is marked by tone only, it may only be a subtle tone difference to indicate the meaning difference between 'little of dog puppy' and 'little dog' talking about the size of the dog!

e) *Demonstratives* also concord. It is good to research such differences as 'this one' *close to speaker*, 'that one' *far from speaker but close to hearer*, 'yonder one' *far away from both speaker and hearer*, 'the (already mentioned) one'. Many languages have a set of four different demonstratives, others have three and still others only two, where the distant one is also used in the anaphoric sense 'the already mentioned one'. In English 'yonder' has fallen into disuse; the article has the anaphoric function.

f) Some *relative* and some *interrogative pronouns* may concord.

g) *Adjectives* and *numerals* may concord, as well as some noun determiners such as 'every', 'many', 'certain'.

h) There might be *special locative indicators*. Originally these are supposed to have been separate locative classes. As remnants of such a class or classes they might look like an anomaly in the language.

2. Establishing classes and assigning numbers

Having researched all the markers, these can now be lined up to determine the number of classes found. It is customary to keep singulars and plurals strictly separate, that is, if a singular class and a plural class have the same markers all the way through, it is still customary to treat them as two different classes. Apart from that, one might try to unite some classes which have overlapping markers. Particularly in the class affix series some nouns may have zero affixes (or no affixes) while others have overt markers (affixes), or the affix form might differ because of certain morpho-phonological processes. Normally each class has a diagnostic consonant, which may sometimes turn into a vowel. It is not always possible to recognize the

class marker in certain fusions with morphemes such as 'every', 'certain', etc.

In Bantu studies it is customary to assign a separate number to each singular class (odd numbers) and to each plural class (even numbers). Welmers (1973:165–167) gives a chart on the forms of the markers found in several language groups and includes a proto-Bantu form. On the basis of the data cited by Welmers one can take a guess as to what class the system in the language might correspond to. A larger collection is found in Guthrie, which I've found interesting to work with. This part of the work is not crucial to understanding how the language works, but I find it 'fun' and it would help comparativists.

Do not have more classes than you find differences in combinations of markers, but even if you have completely identical markers for a singular or a plural class, do not lump these together. Often classes 9 (a singular class) and 10 (its corresponding plural) are identical except for the tone of the associative ('of') marker! This tone is often low for classes 1 and 9 and high for all the others, therefore also for 10. In most Bantu languages classes 1 and 2 are easily identified, since *people* fall into this gender. After that it is more difficult!

3. Establishing the gender

Make a study of how the singular and plural forms of the nouns combine, that is, the nouns of which singular class are found in what corresponding plural class(es). Not all the nouns in one singular class are necessarily found in the same plural class!

The combination of singular with the plural classes are called genders. Each new combination is a different gender.

4. An example

The results of your research might look something like the following. The example is taken from Bariba (Benin). It is a tone language, but tone has been omitted in this particular case; it adds to the demonstrative distinction but not to the class distinction. The demonstratives for near and far, and the anaphoric demonstratives as well as the possessive 'adjectives' differ in tone and by the addition of nasalization to the demonstrative forms cited here. The full set for class 1 is: *wi* 'this', *wí* 'that', *win* 'the', *wín* 'his'; the other sets correspond in regular fashion.

class	noun suffixes	S pronouns	demonstratives	every	certain	who?, what?
1 sg	–u, #	u	wi	winwere	goo	wara
2 pl	–bu, –ba	ba	be	benbere	gabu	bera
5 sg	–ru, –du	ta	te	tentere	garu	tera
6 pl	–nu	nu	ni	ninnire	ganu	nira
7 sg	–a, #	ya	ye	yenyere	gaa	yera
8 pl	–i,–ɛ	yi	yi	yinyere	gɛɛ	yira
14 ne	–bu	bu	bi	binbire	gabu	bira
18 ne	–m	mu	mɛ	mɛmmɛre	gam	mɛra
20 sg	–o, –a, #	ga	ge	gengere	gagu	gera
22 pl	–su	su	si	sinsire	gasu	sira

Notice that the class number assignment is tentative, but a good guess. I did it by examining the data given by Welmers. Class 14 could possible actually be 15 (or 17?); it contains the 'infinitive' forms for the verbs which in Swahili have the prefix *ku–*. Maybe the proto-forms for the relevant class should be **kpu–* which could have velar reflexes in some and labial reflexes in other languages. So the fun begins, and before being sure I would try to consult more books than I have at my disposal right now!

Gender combinations: 1–2, 5–6, 5–22, 7–8, 7–2, 20–22, 20–6, 14, 18 (neuter classes)

To top it off, we need the nouns to prove all these findings!

5. Relevant reading

Much has been written on noun classes, and no attempt is made here to include all references. These are the ones I found most helpful, but read particularly what has been published about languages in your area!

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Studying Morphophonemic Alternation in Annotated Text: Part Two

GARY F. SIMONS

A corpus of texts annotated in multiple dimensions can provide a wealth of material for the formulation and testing of linguistic hypotheses. This article illustrates how this can be true for the study of morphophonemic alternation. In part one (Simons 1988), I showed how the lexical information derived from the detailed interlinear analysis of a text corpus can be used to build a database of analyzed words. Now in part two, the paired lexical and surface word forms from that database are transformed to create reports on the frequencies and environments of the morphophonemic alternations occurring in the data. Looking ahead to figures 14 and 15, you can see the kinds of reports that will ultimately be derived.

The method is presented in a series of numbered steps. Steps 1 through 5 were presented in part one. The discussion below continues with steps 6 through 10. Figure 16 at the end of the article gives a reference summary of all the steps. As explained in part one, the language in the example is Northwest Alaskan Eskimo, also known as Inupiaq.

6. We have now constructed a database of analyzed words from the lexical mappings database of our text corpus. We are ready to commence the series of data transformations that will convert our wordlist into a database of morphophonemic alternations. The first step is to extract just the data of interest from WORDLIST.ITX. Figure 9 shows what we are trying to achieve. We will call this file MORPHO.DAT. In MORPHO.DAT, each record of the wordlist database is reduced to two simple lines. The first is the morphemic representation and the second is the allomorphic transcription. Note that the order of these fields is switched from that in the WORDLIST.ITX file. What we are doing is shifting from the perspective of text analysis (in which we generally begin with surface forms and seek to deduce lexical forms) to the perspective of phonology (in which we generally begin with lexical forms and seek to derive surface forms). In the remaining

discussion we will also adopt this shift in terminology and speak of lexical and surface levels rather than morphemic and allomorphic.

Figure 9. Lexical-to-surface word pairs from word list

```
aiqummit-uti-mik
aiqummir-rut-mik

akuti-naq-tuq
akun -naq-tuq

aiku-tiq-kka-a
aiku-tiq-a  -a

killaq-mun
killa0-mun

akutuq-ai -tla-iq-tut
akut  -chi-t^a-iq-aut
```

The MORPHO.DAT file could be generated by means of a custom program, especially in a text processing language like CC or PTP. It can also be done by means of the general file formatting facility of the TA package (JAARS 1987). This is the approach I will now describe.

Run the *ansq* program and answer *4* at the first prompt in order to select the formatting option. The program will proceed to ask you many questions. Simply hit the ENTER key to accept the default responses except for the questions mentioned explicitly below. For "Record marker?," answer *ref.* Of all of the batteries of options which will follow, it is only the CURRENT FORMATTING group that we want to make changes in. Hit ENTER until the "Change the formatting?" appears. To that, answer *yes.* "Do you want standard format output?"; answer *no.* The program next asks for the field code of the first field you want to output. This is the morphemic representation field, so answer *mr.* We want the field contents output at the beginning of the line, so hit ENTER for the next three questions. Next comes, "Text printed before the field?" Here we want to specify that there should be a blank line before each new record. We specify this by answering *|!|!.* The first *|!* puts the output at the beginning of the line. The second one causes a blank line to be output. Hit ENTER for the text printed after. The next field to be output is the allomorphic transcription field, so answer *at.* Answer *1*

for the "First column?" query, and ENTER for the rest. There are no more fields, so hit ENTER for the "Next field?" query. To the next query, "Is the input already in this order?," answer *no*. Now hit ENTER for the remainder of the questions, until you get to "Answer file name?" This is the file in which the program will save all the answers you have just given. I used MORPHO.ANS when I did it.

Now the *ansq* program automatically calls the *srt* program which will do the actual work. The first thing it needs to do is get your answers to the above questions, so it asks, "Answer file?" Give exactly the same answer, namely, MORPHO.ANS. For "Output file?," say MORPHO.DAT. For "Input file?," say WORDLIST.ITX. Now the program will work for a while. Hit ENTER when it asks for "Next input file?," and the program will then close the output file and return you to the DOS prompt.

7. The next step is to line up the corresponding segments in the lexical/surface word pairs of MORPHO.DAT. This is done by manual editing with your favorite text editor. Just put a space in the surface form to indicate the morphophonemic loss of a lexical phoneme, or a space in the lexical form to indicate the insertion of a surface phoneme. See, for example, number 1 in figure 10 where an instance of both is illustrated. The forms that come out of WORDLIST.ITX are already aligned at morpheme boundaries, so it may look like the job is already done. What generally needs to be done, however, is to move the spaces that already appear within a form so that they occur in the position where the loss or insertion is thought to have taken place. This needs to be done, for instance, in the surface form of the third pair in figure 9. Another problem for the alignment of corresponding segments is the occurrence of digraphs. In the last pair of figure 9, a lexical *s* corresponds to a surface *ch*. The approach we are taking here requires that the phonemes line up one for one. The solution therefore is to change the *ch* to a single character. As shown in figure 10, *C* was chosen in this case.

Figure 10. Formatted listing of aligned lexical/surface pairs

```
001  aiqummit- uti-mik
     aiqummir-rut -mik

002  akuti-naq-tuq
     akun -naq-tuq

003  aiku-tiq-kka-a
     aiku-tiq-  a-a

004  killaq-mun
     killa@-mun

005  akutuq-si-tla-iq-tut
     akut  -Ci-t^a-iq-sut
```

Figure 11 gives the listing of the program used to produce the display format in figure 10. With the MORPHO.DAT file (figure 9) as input, it adds reference numbers and a left margin. The program is written in the PTP language (Simons 1984). In the program listing, all of the quoted material at the beginning of the lines is comments. These may be skipped over when you type the program for use on your own computer. The spacing and line breaks are also not significant. When typing the program, remember that <CR> stands for the ENTER key. Don't type the four character sequence, but press the ENTER key in its place (Simons 1984:34–35). Assuming you name the program file DISPTWO.PTP, it is executed by typing *ptpc disptwo.ptp* at the DOS prompt. The program will ask for the names of your input and output files. You may specify the name of your printer for the output.

Figure 11. PTP program to display two-level wordlist

```
"TO invoke from ptpc command line"  xd.

"TO display two-level word pairs:"  @d{
"   Setup                           "       11b (pm?99w:) cl$ co$
"   Setup example counter           "       10b (pm?99w:)
                                                /     000  $3jq
"   REPEAT for each pair            "       11b (pm'?;
"      Copy blank lines             "            pi<CR>$?cwi:
"      Increment and output count   "            t&ucw7k
"      Output lexical form          "            tcw
"      Output surface form          "            icq           $cw
"      Next pair                    "            i:)
"   Close up shop                   "       11b (pm?99w:) cc b)@
```

8. We next collapse the two line representation of MORPHO.DAT into a single line representation as shown in figure 12. In this representation, a lexical/surface word pair becomes a sequence of lexical/surface phoneme pairs. When the lexical and surface phonemes are the same, the simple phoneme is listed. For instance, in the first morpheme of pair number 2 in figure 10 we see *a*, *k*, and *u* realized without change, followed by a lexical *t* realized as a surface *n*, and a lexical *i* which is lost in the surface form. In the one line representation this is represented as *a k u t/n i/0*.

Figure 12. One-line representation of lexical/surface pairs

```
a i q u m m i t/r - 0/r u t i/0 - m i k //
a k u t/n i/0 - n a q - t u q //
a i k u - t i q - k/0 k/0 a/a - 0/0 a //
k i l l a q/0 - m u n //
a k u t u/0 q/0 - a/C i - t l/^ a - i q - t/a u t //
```

Figure 13 lists the PTP program called COLLAPSE.PTP which transforms the two line format to the one line format. (Refer to the discussion above accompanying figure 11 for instructions on entering the program.) To perform the transformation, run *ptpc collapse.ptp*. The program asks for the input file, which in our example is MORPHO.DAT. The program then cycles through all of the word pairs, displaying each surface form on the screen as it is dealt with. At the end of the list, the program asks for the name of an output file. MORPHO.ONE is the name used in this example. Note that the spaces in the input are converted to *0* and that a // is appended to the end of each word to give a visible sign for word boundary. If ever the

corresponding lexical and surface forms are not exactly the same length, the COLLAPSE program will abort to PTP's immediate mode, with the symbol <ERROR> inserted where the discrepancy was found. Make note of the problem, hit the period key (.) to exit from PTP, use your editor to fix the input file, and then run the program again.

Figure 13. PTP program to generate one-line representation

```
"COLLAPSE.PTP  --  Collapse two-level lexical/surface strings
                   to one line representation"

"TO invoke from ptpc command line     "  xc.

"TO collapse paired forms:            "  @c{
"Setup                                "      11b (pm?99w:)
                                             c1$ 2coCON:$
"Clean up the data                    "      xu
"REPEAT for each pair in list         "      (pm'?;
"   Remove blank lines                "        pi<CR>$?w:
"   Interleave the two forms          "        xi:)
"Output the results                   "      11b c#$ 2cc)#

"TO clean up the data:                "  @u{
"Take out word final spaces inserted  "      (p# <CR>$?ja:)11b
"   by TA program                     "
"Change every space to 0              "      (p# $?jo0$:)11b)#

"TO interleave the two forms:         "  @i{
"Put AX on surface form & display it  "      iq2cwu
"Use V1 to test if both words at <CR>"       v0x
"REPEAT for each char in lexical form"       ( (pi<CR>$?+)t
"   Test for end of forms             "        (pi<CR>$?+)t
"   EXIT IF both forms at end         "        2p#?;
"   Abort with <ERROR> if just one ends"       1p#?/<ERROR>$!;
"   IF same lexical and surface char,"         (p#?
"        THEN output single character,"          k/ $;
"        ELSE output lexical/surface  "          k//$h/ $)
"   Advance surface char              "        tkt:)
"Insert // to terminate word          "       ///$
"Delete surface form                  "       iw)#
```

9. We have now constructed the source file for our study of morphophonemic alternation. The first thing we need to know is what the full inventory of actually occurring morphophonemic alternations is. For this purpose, we can pretend that each lexical/surface pair is a word, and then use the wordlist facility of the TA program (JAARS

1987) to construct an alphabetized list of every alternation in the MORPHO.ONE data file. As an added bonus, the program will give us a total frequency count for each occurring pair, and an index list of item numbers in which each alternation can be found. Figure 14 shows an extract of the output generated from a database of 231 Eskimo words. In that sample, only reference numbers for the first 20 examples are listed. We see for instance that the *a/a* correspondence occurs 608 times, while lexical *a* is lost in 4 cases. With the latter representing fewer than one per cent of the cases of lexical *a*, we are immediately suspicious that the analysis may be incorrect. We therefore look up the four examples given in the reference list to see if there is a plausible conditioning environment, or if alternate analyses would make more sense.

Figure 14. Inventory of lexical/surface alternations

```
231  //    1-20
608  a     2-12
  4  a/0   25,130,202,211
 12  C     10-11,21,60,86,108,129,150,154,161,166,198
  4  C/t   17,43,117,188
 66  q     6,19,24,26,32-34,41,43-44,48,51,56-60,62-63
  1  q/C   216
 38  q/ɬ   15,20,24,34-35,42,45,60,74,77,81,87,97,111
  4  q/k   14,154,166,229
  1  q/p   171
  2  q/'   30,172
  7  q/s   14,55,73,160,173,187,227
  3  q/S   35,190,225
...
 80 Unique selected Words
3784 Total  selected Words
```

To produce an inventory index as in figure 14, run the *ansq* program and answer *1* at the first prompt in order to select the wordlist option. You have no previous answer file, so hit ENTER for the next response. "Is the input in standard format?"; answer *no*. When it asks if you want to change the non-standard format options, answer *yes*. Answer *yes*, to the next three questions: print frequency counts, change referencing, and use line numbers as references. "Do you want all references?" You might want to say *yes* if you do not have very much data, but it is more likely that you will want to say *no* and then specify the maximum number of references in the next query.

Next, answer *no* to the change word selection and change word list options queries.

Now we are to the collating sequence section. You will need to answer *yes* to the "Change the sorting?" query. The default collating sequence which is displayed on the screen contains only the 26 lower case Roman letters. In TA, the collating sequence does double duty. Not only does it tell what order to sort the letters in, it also tells the program what characters are valid word building characters. In other words, any character not accounted for in the collating sequence is simply thrown away on input and regarded as part of the space between words. For our application, at least the slash, zero, and hyphen need to be added to the collating sequence. So do any special characters used to represent phonemes. The first thing the program asks for when you want to change the collating sequence, is the name of a file which already contains your sequence. Press ENTER to say there is none. Next it asks if you want upper and lower case to be considered equal. If you are using capital letters to represent distinct phonemic values, then you must answer *no*. When you do answer *no*, the upper and lower case characters are considered as different characters so you must explicitly place the upper case characters you are using into the collating sequence. It next asks, "Do you want the words sorted from the left side?" Answer *yes* if you want your inventory ordered by the lexical phonemes; answer *no* if you want it ordered by surface phonemes. Next answer *yes* to changing the main sequence. The program now prompts you for the elements of your collating sequence. You may enter a number of elements on the same line; just separate them with spaces. When you press ENTER you will get a prompt to enter more. Enter all your phonemic characters in the order you want them to be alphabetized. Tack the zero, hyphen, and slash onto the end of the list. When you press ENTER without entering any elements on the line, the program terminates the input loop and moves on. Press ENTER for the remainder of the questions to accept all the defaults. The last question is "Answer file name?". Use something like INDEX.ANS.

Now the *ansq* program automatically calls the *wdl* program which will do the actual work. The first thing it needs to do is get your answers to the above questions, so it asks, "Answer file?" Give exactly the same answer, namely, INDEX.ANS. For "Output file?," say INDEX.OUT. For "Input file?," say MORPHO.ONE. Now the program will work for a while. Hit ENTER when it asks for "Next

input file?," and the program will write your output file and return to the DOS prompt. On inspecting the output, if you find entries in which the character on either side of the slash is omitted, it means that you left a character out of the collating sequence. Look up the examples given in the reference list to find out what is missing. Rather than going through the whole *ansq* exercise again, you can edit the answer file (INDEX.ANS) with your text editor. When you get into the file, you will easily spot the collating sequence. Simply add your missing characters where they belong in the list. Rerun the program by typing *wdl* at the DOS prompt.

10. It is not necessary for us to use the index of references produced in step 9 to manually look up all the examples. The TA program can also do that for us, this time by means of its interactive concordance facility. A sample output is given in figure 15. To make such a listing, type *ansq* at the DOS prompt and select option 2 to do an interactive concordance. To the first question, "Previous answer file?," give the name of the answer file you used in step 9, namely, INDEX.ANS. From here on, if you accept the default answer for every question by hitting ENTER, you will get reasonable results. You may, of course, change some of the parameters along the way as well. When it asks for the "Input file?," say MORPHO.ONE. Then it will ask for "Segment to search for?" Type the alternation which you are interested in finding all the examples of. For instance, *#g#* (representing *g* surrounded by word boundaries) was specified to generate the first part of figure 15. When it says, "Next input file?," hit ENTER. Now the program is in a loop in which it will keep asking for more segments to search for. Consult the inventory you produced in step 9 to be sure that you specify all the alternations that are relevant in accounting for a particular lexical or surface phoneme.

Figure 15. Concordance of alternations with lexical *g*

```
------------------------------ g ------------------------------
6          u t // a u k - m a g u // p a u n ɣ a q/0 - t -
19       q/h - a k - n i k/g - g u u q // k a y u q t u q/0
24       - g/ɣ a q/0 - g/ɣ i - g a i // t a - i m m a - a/0
26       0 - q q a a q - l/ɬ u g u // a m i - k s a q/ɣ - a
32        t a - a m n a // t i g l i k - l/h u n i // a q u
33       l l i q/0 - u -/p l u g u // i t n a q/0 - g/ɣ a
34       - g/ɣ a q/0 - g/ɣ i - g a a t - a m i // p a q n a
41       s i q u m m i t - l u g u // n i p l i q/0 - u t i
...
66 occurrences

------------------------------ g/ɣ ------------------------------
15        ɣ // a k u t u q/0 - g/ɣ u u - r u a t // q a n u
20       t u q/0 - u r a q/ɣ - g/ɣ u u q // i $ u k/0 - i c
24       a m i - ?/r ?/r ?/i - g/ɣ a q/0 - g/ɣ i - g a i //
24       ?/r ?/i - g/ɣ a q/0 - g/ɣ i - g a i // t a - i m m
34        g u // i t n a q/0 - g/ɣ a q/0 - g/ɣ i - g a a t
34       n a q/0 - g/ɣ a q/0 - g/ɣ i - g a a t - a m i // p
35       - g/s u u - n i q/0 - g/ɣ a a t // n a u - t i/0 -
...
38 occurrences

------------------------------ g/s ------------------------------
14       u v v i q/0 - a q/0 - g/s u k - g/k a m i ɣ // a k
55       u t u/0 q/0 - s/c i - g/s u u - s/s u k n a q - t
73        t/s u t // n i ɣ i - g/s u k/0 - l i q/0 - a q -
160       - t u q // i k * i - g/s u k - l/h u n i // i $ u
173      u t u/0 q/0 - s/c i - g/s u k - t u n i // q a l u
187      a k u t i/0 - s/c i - g/s u k/0 - i q/ɣ - n/$ a q
227       l u g u // n i ɣ i - g/s u u - g a a t - s u l i
7 occurrences
```

In the resulting concordance, every instance of a specified alternation (along with its reference number) is printed in context in a vertically aligned list. By looking at what is in common on either side of the alternation, we begin to formulate hypotheses about the factors which condition the alternation. For instance, the second part of figure 15 shows instances of the *g*/# alternation. (*g* is a voiced velar fricative, and # represents a voiced back velar fricative-dotted–*g* in the practical orthography.) We note that every instance but one is at a morpheme boundary following a lexical *q* (which is a back velar stop). That is a very plausible conditioning environment, so we formulate the hypothesis that the voiced velar fricative is realized as a

voiced back velar fricative following a back velar stop. In the one exception, number 24, we see that the analyst was in question about the lexical form of the preceding morpheme. From what we've learned so far, we assume that the preceding morpheme must end with the *q/0* alternation, so we go back to our printout of the MORPHO.DAT database and write in a note to that effect on example 24.

The third part of figure 15 gives another example. There we see a pattern that the *g/s* alternation occurs between *i* and *u*. Since there is a pervasive pattern throughout the phonology of palatalization of consonants following *i*, the conditioning environment seems plausible. However, when we look at the one exception in number 15 we begin to wonder. The following *u* is present, but not the preceding *i*. The examples of *g/g* show that the following *u* by itself is not sufficient to explain *g/s*. Looking again at the seven examples for *g/s*, we observe that only two posited lexical morphemes are involved-*guk* and *guu*. Might these actually be lexical *suk* and *suu?* To test that hypothesis, we need to go back to our WORDLIST.ITX file and find all the records with *–guk* or *–guu* in the *\mr* field to find all the other surface forms for which these lexical forms have been posited. The record selection facility of TA offers a good way to do this. We might need to do further searches in the original annotated text corpus to see the situational contexts for the suspect forms in order to test another possible hypothesis-that there are in fact two different morphemes, lexical *–guk* and *–suk*, which have gotten mixed together.

In these few pages it is impossible to teach everything it would be necessary to know in order to become a virtuoso at this kind of hypothesis formulating and testing. I hope, however, that the examples have been suggestive enough to get you started in treating your own data in this way. For the sake of reference, the ten steps of data manipulation described in both parts of this article are summarized in figure 16 as a step–by–step recipe.

Figure 16. Step-by-step summary

1. Use *itp* to annotate a text corpus, mapping from baseline text to allomorphic transcription to morphemic representation (figures 1 and 2).

2. Run *lex–extr –f convert.ctl wordlist.sfl*, where WORDLIST.SFL is illustrated in figure 3 and CONVERT.CTL in figure 4.

3. Use a text editor to search for *;* in WORDLIST.SFL. If found, make multiple records for the word.

4. Run *itprep –f figure5.ctl –m figure6.itm wordlist.sfl*, which produces WORDLIST.ITX illustrated in figure 7.

5. Remove comment markers (\#) from FIGURE6.ITM and run *itp wordlist.itx*, the result of which is shown in figure 8.

6. Run *ansq* of the TA package to transform the input file WORDLIST.ITX into MORPHO.DAT, a file of lexical–surface word pairs illustrated in figure 9. (See section 6 of text for instructions on how to answer questions in *ansq*.)

7. Edit MORPHO.DAT to align the corresponding segments of the paired lexical and surface word forms. Run *ptpc disptwo.ptp* (figure 11) to make a proof printout as in figure 10.

8. Run *ptpc collapse.ptp* (figure 13), with input MORPHO.DAT and output MORPHO.ONE, to collapse the lexical and surface word pairs into a stream of lexical/surface phoneme correspondences (figure 12).

9. Run the wordlist option of TA to generate an inventory of all occurring lexical/surface alternations, complete with frequency counts and an index to examples (figure 14).

10. Run the interactive concordance option of TA to generate aligned listings of alternations of interest in their contexts (figure 15).

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☞ *COURSE WORKSHOP ON CADA* ☜

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Why Test Intelligibility?

BARBARA F. GRIMES

'If you in a tongue utter speech that is not intelligible, how will anyone know what is said? For you will be speaking into the air. There are doubtless many different language in the world, and none is without meaning; but if I do not know the meaning of the language, I shall be a foreigner to the speaker and the speaker a foreigner to me.'

The Apostle Paul argued thus with the Corinthian Christians in the First Century A.D. (1 Cor. 14:9–11) because meaningful communication was basic for him (B. F. Grimes ms.). And so it is, or should be, for governments interested in communicating with their citizens; for educators preparing materials, training teachers, and teaching students; for community development agents trying to bring about improvements in agriculture, health, or literacy; for counselors dealing with family or other social problems; for linguists, anthropologists, and other scholars engaging in research; and for missionaries and Bible translators trying to communicate life-changing concepts.

Adequate communication in a multilingual setting involves a responsible language choice by the communicator. Decisions about which language to use, however, are often made on the basis of what is easiest for the communicator, rather than on what communicates most effectively with the hearers. The communicator probably already knows, or has to learn, the national language or lingua franca, and literature may already exist in that language. It may seem an unnecessary burden to have to learn the vernacular speech of his audience as well.

For effective communication, however, language decisions need to be based on accurate evaluations of how well people understand their second language or other dialects of their first language, and on attitudes in the various segments of society about the acceptability of using other languages or dialects they may understand.

Suggestions about how to evaluate degrees of second language proficiency in a community have been given by B. F. Grimes (1986a) and Quakenbush (1986). Methods for evaluating language attitudes have been described by Agheyisi and Fishman (1970). The relative importance of comprehension versus language attitudes has been

discussed by B. F. Grimes (1982, 1985a). How to test intelligibility among related dialects and languages has been described in detail by Casad (1974).

This paper attempts to show that other methods for getting at dialect intelligibility which have the apparent advantage of being faster than intelligibility testing, and which may have validity for some other purposes, are inadequate for evaluating dialect intelligibility. Because of the indirectness of their approaches to intelligibility, the results from those methods still need to be verified for each dialect pair separately by the more direct method of intelligibility testing as described by Casad.

PUBLISHED LISTS OF LANGUAGES AND DIALECTS

Is it not possible to get an accurate picture of what are separate languages and what are dialects of those languages by using lists of languages and dialects that are already available? Unfortunately, such lists often contain inaccuracies.

'Dialect' versus 'Language.' The use of the term 'dialect' may not indicate that the form of speech is inherently intelligible with another form of speech, but rather may be an example of popular use, which attributes dialect status to anything that is not a major language. Similarly, the term 'language' may be used in a popular sense only for languages with official status which have many speakers.

Ethnic Names. Names of ethnic or people groups or tribes do not always correlate on a one–to–one basis with names of languages. An ethnic name may include several languages, some of which may not even be related, or closely related, to each other. The Yugur official ethnic minority in China, also called Yellow Uighur, is made up of two distantly related languages (Jan–Olof Svantesson, personal communication): West Yugur, which is a Northern Turkic language (Voegelin and Voegelin 1977.340), and East Yugur, which is a Mongolian language (Poppe 1970). Both are distinct from Uighur, which is Eastern Turkic. Similarly, the Black Bobo (Bobo Fing) and Red Bobo (Bwamu and Boomu) of Burkina Faso and Mali are in the Mande and Gur families of Niger–Congo, respectively. These and other similar cases are sometimes merged or confused in lists.

On the other hand, distinct ethnic names may identify groups whose speech is inherently intelligible to each other, and hence are linguistically dialects, but which are listed as separate 'languages'

because of political and other cultural differences, such as the Sotho, Pedi, and Tswana of southern Africa, or the Zulu, Swazi, and Xhosa, also of southern Africa.

Even for groups which do have a one–to–one correlation between people names and languages, there may be distinct names for the people and their language; for example, for Mamprusi and Mampruli, Kusasi and Kusaal, Kasena and Kasem, Dagbamba or Dagomba and Dagbani, Dagaaba and Dagaari, all of Ghana. Some sources list such pairs as two languages, as for Tamprusi and Tampulma of Ghana (Voegelin and Voegelin 1977.151).

Traditional Names. Historic names may continue to be used for groups, with the separate varieties being referred to as 'dialects,' even though the traditional name may include several inherently unintelligible forms of speech. This is true of the names the Aztec Empire gave to other Indian groups in Mexico whom they ruled, and who paid them tribute. Thus, all Zapoteco speakers are commonly thought to speak one language, although intelligibility testing has distinguished about 55 inherently unintelligible forms of speech. The same is true of the Mixteco, which have about 33 distinct forms, Chinanteco with about 14, Mixe with about 8, and the Aztecs themselves (Nahuatl) with about 15 distinct forms. The Chontal of Oaxaca and Chontal of Tabasco inherited the same name (the Aztec's word for *barbarian*), even though they are in the unrelated Hokan and Mayan language families, respectively. If the Aztec Empire had called the speakers of the distinct forms by different names, people today would probably refer to them as separate languages. As it is, they are referred to as 'dialects.' By using only the traditional names, Mexico is reported to have about 52 languages, but as the result of intelligibility testing, about 240 inherently unintelligible speech varieties have been distinguished (Bradley 1968, Kirk 1970, Casad 1974, Stoltzfus 1974, Egland 1983).

Official Lists. Government lists may not accurately distinguish dialects, languages, and ethnic groups. China lists 55 official ethnic minorities, but some Chinese linguists are recognizing that some of the official minorities include more than one language. This is illustrated by the Yugur example, given above, and by others. The Dai official minority includes at least the following languages: White Tai, Black Tai, Tai Neua, Shan, Lue (Svantesson, personal communication), and possibly Kang. On the other hand, among the 54 official ethnic communities recognized by Vietnam, Lue is separated

from the official Thai ethnic community, which includes the White Tai, Black Tai, and Red Tai languages. The Nung and Tho languages in China are not distinguished officially, but are probably both included in the official Zhuang ethnic minority, whereas in Vietnam Nung and Tho are officially separate ethnic communities.

Different Countries. The same language may be referred to by different names in different countries. Guere is the name used in Ivory Coast for what is called Krahn in Liberia. Muslim Chinese, called Hui in China (the name of the people), are called Dungan in the USSR, and Khoton in Mongolia (names for both people and language). Cantonese is called Tay Nung or Chinese Nung in Vietnam. Vietnamese is called Gin or Jing in China.

Union Languages. There are 'union' or artificially constructed languages whose ethnic groups are composed of speakers of related dialects or languages that have varying degrees of intelligibility with each other. Literature may exist in the union form and also in some of the separate dialects or languages. It is commonly assumed that speakers of all 'dialects' in the group can understand the union literature, even though some may have only marginal understanding of it. The union language may be used only in written form, or it may also be used as a standard form or lingua franca for some oral purposes, but without native speakers. Kalenjin of Kenya includes Nandi, Terik, Kipsigis, Keiyo, Tugen, Marakwet, Cherangany, Sabaot, Okiek, and Pakoot of Kenya, and Tatoga of Tanzania, although only the first five listed may be able to adequately use Kalenjin literature. Beti of Cameroon includes Bebele, Bulu, Eton, Ewondo, Fang, Mangisa. Shona of Zimbabwe includes Karanga, Zezuru, Korekore, Manyika; Ndau is only partially intelligible. An attempt at union literature for Lango and Acholi of Uganda, called Lwo, was not successful (Nida 1972.3).

Language Standardization. The need for language standardization often results in all dialects and languages closely related linguistically or culturally to the standard language being treated as dialects of it, even though some of them may be inherently unintelligible with the standard variety (B. F. Grimes 1986c). There are many examples of this throughout the world, such as Malay, Indonesian, Javanese, Hindi, Arabic, Rumanian, Portuguese, Italian, German (Voegelin and Voegelin 1977.179, 178, 166, 305, 295, 296, 297, 146).

Creole Languages. Creole languages present problems similar to that of standard and non-standard dialects, with the added complication

that in countries where the standard language on which the creole is based is also the national language, there is often a spectrum or continuum of varieties from the extreme creole, or *basilect*, on the one hand, to the standard language, or *acrolect*, on the other. Speakers use those varieties as appropriate in different social contexts, but different individuals may control different parts of the spectrum. All speakers may not be able to use or understand the standard variety. Sometimes the basilect is different enough from the acrolect to be inherently unintelligible with it. There may also be dialects of the basilect influenced by the original ethnic languages of the families of the speakers, as for Hawaiian Creole English (Michael Forman, personal communication; Carr 1972.34–43). However, creole languages are usually considered to be only 'broken' forms of the standard language, and the false assumption is made that all speakers can understand the standard variety.

Geographic Labels. Language names may contain geographic terms which can give the impression that all varieties from that region which share that label are related or are dialects of a single language, even when some are not. Muinane is a language of Colombia, sometimes confused with Muinane Huitoto, which has historically been in the same region. Muinane is related to Bora, which some linguists consider to be related to Huitoto (Voegelin and Voegelin 1977.352) and others do not. However, Muinane and Muinane Huitoto are distinct languages (J. Walton, personal communication).

Other geographic names such as *Bihari* for speakers from Bihar state in India, and *Rajasthani* for speakers from Rajasthan in India are applied in an imprecise way to dialects spoken in those states.

On the other hand, language names may be used in a geographic sense: *Buton* may be applied outside of Southeast Sulawesi, Indonesia, to anyone from that region, and *Javanese* may be used outside of Java for anyone from that island, whether they speak Butonese or Javanese or some other language.

Village Names. Early lists of languages in Papua New Guinea and Irian Jaya contained some village names which did not necessarily represent separate dialects or languages.

Alternate Names. Lists may include variant names for the same language as though they were separate languages, such as Aucaan and Djuka of Surinam (Voegelin and Voegelin 1977.143). Early explorers in Papua New Guinea found some language groups that did

not have names, so they arbitrarily assigned to them their own word for house. Other explorers assigned to the same languages their word for man. In other places, a word by which dialects differ has been assigned to each as its name. Some groups have been given derogatory names (Chontal, Araucanian, Auca, Jivaro), which in some cases have become the official name for them. Nearby groups often use different names for a given neighbor, while that neighbor may use still a different one for itself. Other groups are distinguished by the color of the women's blouses or the color of headgear, such as the White Tai, Red Tai, Black Karen, Striped Karen, Blue Meo, Green Meo, Flowery Miao, even though such distinctions may or may not coincide with language differences.

Census Lists. Fasold (1984.23) describes some of the problems with language names found in census information. 'For some people, it may be the first time they have even had to think about what language they speak. The answer is likely to be based more on the desire to be associated with a prestige language, to appear patriotic, or to claim allegiance to a local ethnic group...some people have a very local self–concept. Not knowing a name for their language, and unwilling for one reason or other to name one of the major languages, they give the name of their caste, locality, or even their occupations as the name of their language. It is now up to the census officials to decide how to count such a response.'

Extinct Languages. Lists may say that certain languages are extinct which are actually extant, such as Urarina of Peru, Chamicuro of Peru, Apiaca of Brazil, Culina of Brazil and Peru, Saliba of Colombia and Venezuela (Voegelin and Voegelin 1977.22, 24, 95, 282, 132). Others may be listed as nearly extinct which are still very much alive, such as Sea Island Creole (Gullah; Voegelin and Voegelin 1977.143).

Language Groups. Linguists sometimes use the term 'language' in a larger sense than a group of inherently intelligible dialects, for what is really a group of related languages. This usage may be based on linguistic similarities, ethnic relationships, language standardization, tradition, or a combination of these. Some linguists refer to Quechua of South America as a single language (Voegelin and Voegelin 1977.21). Other examples of this usage are Berber (Gunnemark and Kenrick 1985.11, 36, 38, 40, 42, 55), and Maya (Gunnemark and Kenrick 1985.39).

Inherent Intelligibility. Linguists may treat any degree of inherent intelligibility as indicating that variants are dialects of a single

language. Voegelin and Voegelin (1977.357) say regarding Zapoteco of Mexico, 'Longacre (1964) lists seven different languages, among some of which (according to de Angulo and Freeland (1933)) there exists mutual intelligibility, and hence different dialects rather than different languages.' As already mentioned, lack of adequate intelligibility distinguishes 55 Zapoteco languages, rather than marginal or limited intelligibility uniting them.

It seems clear from these examples that it is not possible to use lists of 'languages' and 'dialects' to indicate where there is adequate intelligibility and where there is not. It is necessary to do further investigation in order to understand the real relationships.

COMPARING WORD LISTS

Probably the most frequently used method for establishing dialect closeness in survey situations is through comparing word lists. Swadesh (1950, 1952, 1953, 1955) proposed lists of 100 and 200 words that he said were those words least likely to have changed through time. He was primarily trying to determine the time depth since languages diverged, believing that linguistic change is uniform through time from one language to another. Most linguists have rejected the idea that there is such a regular relationship of time to linguistic change (Gudschinsky 1955, 1956a, 1956b, Hoijer 1956, Rea 1958, among others). However, his lists and modifications of them have been used in many parts of the world for dialect and language comparisons.

Swadesh suggested that the dividing line between dialects of one language and separate languages was 81% lexical similarity, based on averaging what was known about some languages through comparative linguistics and through archeology. He suggested that comparison of probable cognates by inspecting word lists of dialects and languages could give a resulting percentage of similarity, called 'lexicostatistics,' that would approximate a figure for true cognates, arrived at through the comparative method, and would be a faster way to determine language relationships than the comparative method.

"Swadesh was primarily trying to determine the time depth since languages diverged. . ."

Comparative studies have been undertaken mainly to find the closeness of phonological systems, and to understand the phonological processes which take place as languages and dialects diverge. Swadesh invented lexicostatistics to try to find the time since divergence. Comparing word lists to find a percentage of lexical similarity and therefore to approximate intelligibility between dialects is a still different use.

Simons (1979) compared the results of lexical similarity and intelligibility testing in ten surveys around the world. He concludes, 'Inspection of the trend line shows that above 60% similarity, intelligibility steadily rises. Below 60% the intelligibility fluctuates and shows no steady trend.' (1979.90-94). He suggests, '...for every percentage point which lexical similarity increases beyond 40%, the degree of intelligibility increases by one and two–thirds percent.' (1979.90).

However, although these guidelines are true on the average for those surveys, they are not necessarily true for any given pair of dialects being compared in those surveys. The spread of the intelligibility scores for dialects with 80% lexical similarity ranges from 30% to 79%; for 70% lexical similarity, the range of intelligibility is from 9% to 79%; for 60% lexical similarity, the range is from 0% to 67% (Simons 1979.64,88). Therefore, it is not yet possible to predict dialect intelligibility with any degree of certainty by only comparing lexical similarity between two dialects.

Joseph Grimes (1988) has shown concerning the surveys Simons evaluates and other data from the Philippines that, 'Of these eleven sets of scores, most of the data that contribute to high correlations come in the area of low vocabulary similarity predicting low intelligibility. At the higher end of the scale, where adequate intelligibility is involved, only two sets of scores out of the eleven, those of Mazateco and Trique, show a strong correlation.'

Some investigators who have hoped to use lexicostatistics as a means of getting at intelligibility have not been fully aware of the variety of serious problems connected with collecting word lists, comparing them, and interpreting the results. Other investigators who have been aware of the subjective nature of these comparisons have rejected lexicostatistics completely. Still other investigators feel that as long as

one is aware of the possible pitfalls, word list comparisons can give at least an initial indication of relationships, providing an indication of which languages probably do not need intelligibility testing (below 60% lexical similarity), and which ones do (above 60%). What, then, are these pitfalls that one should be aware of?

PROBLEMS IN COMPARING WORD LISTS

The Comparative Method Versus the Inspection Method. Some have argued that the inspection method for possible cognates does not select unrecognizable cognates, and so is a closer approximation to intelligibility than is a percentage of true cognate similarity. They also argue that the inability of the inspection method to weed out loan words is an advantage, because loan words correlate with understanding. However, Joseph Grimes points out (personal communication) that loan words when taken into a language fill in a hole in the semantics of the borrowing language, and loans often shift meaning in the new language to fill that hole. An example of that kind of semantic shift is given by Abdulaziz Mkilifi (1978.143) in Fasold (1984.57), in which a bilingual speaker used the English word 'fruit' in a Swahili sentence. When asked why he hadn't used the Swahili word *matunda*, he answered that *matunda* is picked from trees and 'fruit' is bought at a market.

Another problem is that if a list in one dialect uses a loan word and another dialect list does not, the loan can lower lexical similarity where the loan has replaced a true cognate, although that cognate may still be known by the speakers who use the loan; yet the loan may not be known by speakers in the other dialect. Loans do not come into a dialect with the same uniformity as genetic features, but may be known only in one region or by certain classes of people. Loans have no relationship to the genetic closeness of dialects in their phonologies, grammars, discourse features, or the remainder of the lexicon, whereas true cognates do have a relationship to that overall closeness.

The subjectivity of the inspection method results in investigators interpreting possible cognates differently. Some count only those words they think are real cognates. Others count only those they think speakers would understand. Others count only those that are identical. Gudschinsky (1956b) and others have proposed procedures for reducing some of the subjectivity involved.

Now that computers can be used in true comparative studies (Frantz 1970, Simons 1984), those more careful comparative studies no longer have the disadvantage of taking more time than can be afforded on surveys. They do have the advantage of being a more accurate indicator of genetic closeness than merely inspecting word lists for possible cognates. (See the appendix to this paper for cognate percentages of some European languages.)

The Lexicon as an Adequate Sample of a Language or Indicator of Intelligibility. There is a question as to whether or not a word list is an adequate sample of a language or dialect. Word lists ordinarily contain stems, whereas affixes, function words, word order changes, and discourse markers which may seriously affect meaning are not indicated. For example, an affix which indicates 'negative' in one Quechua dialect indicates 'positive affirmation' in a nearby Quechua dialect. The form *kowancu* 'he gives to us' in North Bolivian Quechua, means 'they give to me' in South Bolivian Quechua, because of fused subject–object person affixes with the same form having different meanings (B. Wrisley, personal communication). This kind of difference in Quechua may be one reason why some Quechua dialects of Peru with as high as 95% and 96% lexical similarity (Torero 1970) have been found to have significant difficulties in intelligibility with each other (D. Weber, personal communication). Similarly, although lexical similarity percentages are high between many Philippines languages, differences in function words can distort comprehension between them (C. Walton, personal communication). Similar situations are reported for Guarani languages of South America (L. Bridgeman, personal communication) and Mayan languages of Central America (S. Echerd, personal communication), where lexical similarity percentages are high, yet grammatical and discourse differences hinder communication. Such hindrances in Quechua and Malay dialects have been described by speakers as, 'I understand all the words, but I don't have the vaguest idea what they are saying.'

Although Spanish and Portuguese have 89% lexical similarity (Rea 1958), intelligibility is hindered at least partially by complex morphophonemic changes in Portuguese which make Portuguese difficult for Spanish speakers to understand. Words that are true cognates can be unrecognizable to a Spanish speaker who has not had contact with Portuguese; Portuguese speakers have less difficulty with Spanish.

Language attitudes can also affect intelligibility (Wolff 1959, in Fasold 1984.148–149) even if lexical similarity is high. This can happen when speakers of a higher status variety claim not to understand a lower status variety, although speakers of the lower status variety do understand the higher. Even if the possibilities of unequal two-way inherent intelligibility and one-way bilingualism are ruled out, it is still possible that language attitudes can be strong enough for other reasons to effectively hinder intelligibility between dialects.

A Word List as an Adequate Sample of the Lexicon. There is also a question as to whether or not a given word list of 100 or 200 words is an adequate sample of the total lexicon of a language for purposes of comparing that lexicon with the lexicon of another language (Gudschinsky 1956a). If the Swadesh list truly represents words that are less likely to change through time, as he claimed, then the total lexicon of two languages would be less similar than the lexicostatistics indicate. The lists are not randomly selected, but carefully screened. If the lists are modified very much to eliminate culturally or geographically irrelevant terms, then the resulting percentages are less meaningful when compared with the results from other lists which have not been modified, or have been modified in other ways. This is illustrated in an extreme way by the following carefully selected lists, which could seem to indicate that American English and British English are not related:

American English	British English
flashlight	*torch*
pitcher	*jug*
windshield	*wind screen*
car trunk	*boot*
car hood	*bonnet*
station wagon	*shooting brake, estate wagon*
truck	*lorry*
thumb tack	*drawing pin*
cookie	*biscuit*
biscuit	*scone*
French fries	*chips*
supper	*tea*
bedtime snack	*supper*
coffee break	*elevenses*
elevator	*lift*
freight	*goods*
conductor	*guard*
switches	*points*
ties	*sleepers*
(railroad) car	*carriage*
napkin	*serviette*
diaper	*napkin*

Same Informant, Differing Lists. The same informant can give differing word lists at different times. The area of meaning covered by apparent equivalents in two languages is often not identical; there is partial overlap and some difference in meaning. The informant may give one synonym one time, and another at a different time, with differing overlap in meaning. One time he may give a specific equivalent, another time a generic equivalent. One time he may give a loan word, another time a vernacular word. One time a regionalism, another time a more universally known word. Different elicitors may evoke different responses from an informant because of the elicitors' ethnic or linguistic affiliation, personality traits, or cultural status. Different settings for the taking of lists, such as school or home, may evoke more formal or more informal responses. Whether or not other people are present and who they are can bring about different responses. Different moods of the elicitor or responder may influence different responses.

Different Informants, Differing Lists. Different informants from the same dialect can give lists that differ somewhat because they speak slightly different idiolects, or for any of the above mentioned reasons.

Language of Elicitation. The language of elicitation may cause different responses. It may elicit more borrowings from itself than would ordinarily be used. If the language of elicitation is related to the language being investigated, it may elicit cognates that are not well known, or cognates that have shifted meaning, but which are given because of the obvious form relationship, even though they are not a close meaningful equivalent. Different languages of elicitation used for collecting lists from different informants in the same dialect or from different dialects may cause different responses.

Difficulty in Finding a Single Term. Hoijer (1956) pointed out that a single term in the language of elicitation may not have a single equivalent in the dialect being investigated. In that case, an informant may not be able to give a response, or may arbitrarily choose one of several possible partial equivalents.

Same Word for Several Items. Barbara D. Grimes (personal communication) reports that in Indonesia there was a problem with the same term being used for several body parts, such as 'palm of hand' and 'sole of foot,' or for 'red' and 'blood,' or 'water' and 'river,' where these items are distinguished in the language of elicitation but not in the language being investigated.

Misunderstandings. Jack Keels (personal communication) found after he learned the Guayabero language of Colombia that an earlier list in Guayabero taken by someone who did not know the language differed by 25% from his list. In one instance, the investigator had asked for the word for 'amarillo' (yellow), and the informant had responded with the word for 'armadillo.' In another instance, the investigator had asked for the word for 'man,' but because the language has separate words for 'human being,' 'male,' and 'Indian man,' the informant chose a different one from that chosen in another dialect. Lists taken by investigators who know at least one of the dialects are more likely to be reliable than those taken by investigators who do not.

Gestures. Carolyn Miller, (personal communication) reports that in Sabah, Malaysia, different gestures by different investigators resulted in different responses, such as, 'to throw overhand' or 'to throw underhand,' 'to throw far' or 'to toss.' Pointing gestures led to different specific words: 'front of neck' or 'back of neck,' 'throat' or 'larynx,' 'ankle' or 'foot.'

OTHER METHODS OF COMPARISON

Phonostatistics. Joseph Grimes (1959, 1964) developed a method for calibrating the degree of phonetic change or distance between dialects, based on results of the comparative method. It calibrates the degree of phonological closeness of cognates, rather than counting identical, similar, and dissimilar cognates as equal, as in lexical similarity comparisons. McKaughan (1964) and Ladefoged (1968, 1970) used similar methods. These are described in Casad (1974.119–122) and Simons (1977.155–185).

Complexity of Phonological Change. Agard (1975) has suggested that the complexity of phonological change that has occurred with language divergence may correlate with dialect versus language distinctions, and would offer a less subjective means of getting at those distinctions than lexicostatistics or intelligibility testing. He suggests that simple phonological changes, such as a change from voiced to voiceless, stop to fricative, or the dropping out of a sound, may not separate languages, whereas more complex changes, such as one change on top of another or changes tied to different environments in different parts of the original speech community, may cause difficulties for the hearer, and thus point to separate languages. Margaret Milliken is investigating the relationship of Agard's suggestions to the results of intelligibility testing.

Grammar and Discourse. Joseph Grimes (personal communication) has suggested that the tests devised by Weber and Mann (1979, 1980) for finding out if dialects are candidates for computer assisted adaptation of literature from one dialect to another might be used as a survey technique to arrive at a better calibration and understanding of grammatical and discourse differences between dialects than is possible by comparing vocabulary lists alone. They would yield a composite measure based on phonological, morphological, lexical, and syntactic changes required by the dialect adaptation process.

Sentence Repeat Test. Tests in which a subject repeats sentences from another dialect have been thought to correlate with intelligibility between the dialects (Casad 1974.60–61, Simons 1979.30). Crawford found in Mexico, however, that the sentence repeat test did not correlate well with understanding, and that for highly intelligible dialects it was more like a mimicry test than a gauge of understanding (Casad 1974.61). Teachers of phonetics have observed that individual ability to mimic what we do not understand varies. It would not be possible to distinguish that ability from intelligibility in this kind of test.

Ask the Informant Test. Asking informants which dialects are more intelligible and which are less intelligible can furnish important information for a survey (Casad 1974.122–124, Callister 1977.189–191). It would not help, however, for dialects the speakers do not know about, such as Nahuatl dialects scattered around central and southern Mexico. Charles and Barbara D. Grimes (personal communication) found on Buru Island in Maluku, Indonesia, that initially everyone insisted that all residents of the island spoke the same language, although some preliminary lexical comparisons showed only between 49% and 66% lexical similarity. Several months later some people began to reveal that they had to resort to using the lingua franca with people from certain areas in order to communicate adequately, because the variation in speech was too great. Furthermore, informant reports of intelligibility may not distinguish bilingualism from intelligibility.

INTELLIGIBILITY TESTING NEEDED

Direct Evaluation. All of these suggestions for comparing phonological, lexical, grammatical, and discourse differences between dialects, or for attempting to calibrate intelligibility by mimicry or asking for opinions, can only be approximations to intelligibility. In a survey situation in which an investigator interacts with only a small

sample of the language it is not likely that he can calibrate how much grammatical versus lexical versus discourse versus phonological differences influence intelligibility between two dialects. For a given pair of dialects, grammatical differences may be the crucial factor, whereas for another pair, lexical differences may be crucial, and for a third pair it may be a complex interaction of several factors. Initial indications of dialect intelligibility from these other methods can only be verified by carrying out actual intelligibility tests. Direct measurement of what is in question is more useful than only measuring other things which have varying and undetermined degrees of relationship to that.

'Define and Group Languages and Dialects Rationally' (Casad 1974.1). If there are multiple dialects, it may be that separate literature is not needed for each, but neither can all dialects be covered by materials in only one dialect. Intelligibility testing gives a sound basis for grouping and separating dialects and languages.

Strategic Centers. Joseph Grimes (1974) has developed a method for finding linguistic centers from the results of intelligibility testing, and this has been refined by Simons (1979.47–55). Considering only sociolinguistic centers or practical factors such as accessibility and openness without knowing what are the centers of intelligibility can result in some dialects being left out of literature programs, or in extra programs needing to be carried out later in order to cover them.

Pidgins and Creoles. Lexical similarity percentages can be misleading for pidgins and creoles, which may have high similarity percentages with each other or with their acrolect, but which are based on grammatical structures very different from their acrolect. It is necessary to do intelligibility testing in order to know how well they understand other creoles, or the acrolect, or standard language.

Flint (1979, in Fasold 1984.103–105) tested intelligibility of 17 dialogues in Norfolk Island Creole English by a native speaker of Australian English who was also a linguist familiar with other creoles, but not with that of Norfolk Island. For each dialogue he then compared the percentage of words in the dialogue which were lexically similar to English and which also had meanings close to their standard English meaning. He then compared those percentages with the results from the intelligibility testing. The intelligibility ranged from 32.5% on a dialogue with 89.5% lexical similarity, to 79.7% on a dialogue with 93.9% lexical similarity. The lexical similarity ranged from 84.6% with intelligibility of 53.4%, to 97.8%

with intelligibility of 74.9%. Lexical similarities of 93.5%, 93.6%, and 93.9% went with intelligibility of 65.6%, 58.8%, and 79.7%, respectively. Because this test compared intelligibility and lexical similarity of the same language sample, rather than comparing lexical similarity of a separate vocabulary list, it demonstrates several things for creole languages, at least: there is variation in both lexical similarity and intelligibility within a language depending on the specific sample of the language chosen; there is variation in the relationship of those two factors to each other because of other factors not compared, such as grammar, discourse, and progressive learning. (The correlation between order of testing and intelligibility (*r*–correlation of 0.825), was actually closer than the correlation between lexical similarity and intelligibility (*r*–correlation of 0.56), something not mentioned by Fasold. (J. Grimes, personal communication)). Intelligibility increased as the testee heard more of the language. It is important to note that lexical similarity of frequency of occurrence in a dialogue is different than lexical similarity of occurrence in a select vocabulary list.

Carolyn Miller (personal communication) has reported situations in Sabah, Malaysia, where certain dialects appear to be Dusunic in lexicon but Murutic in grammar. Other mixed situations are reported for the coastal plantations of Guatemala. These could be pidgins or creoles which have not yet been identified as such. Trying to determine intelligibility for them by comparing lexical similarity alone could be misleading.

Hall (1966.124) points out, 'for most languages of the earth, we do not know their previous history, and have no way of knowing whether or not—especially in the case of apparently distantly related languages—a stage of pidginization and creolization may have entered into the process of differentiation. This would seem especially likely in certain regions, such as Melanesia, where it is suspected that pidginization may be endemic as a linguistic behavior pattern; but who can tell whether similar processes may not have taken place elsewhere, in places and times no longer accessible to use by historical record?'

Geographical Extent of the Language. Intelligibility testing makes the investigator aware of how far intelligible dialects extend, so he can determine adjustments in orthography, vocabulary, or grammar that should be made in literature for all those dialects to use (C. Walton, personal communication).

Dialect Chains. Dialect chains are made up of a series of dialects each of which has some intelligibility with those next to it, but which may not be able to understand those farther away, such as Arabic dialects which extend from Senegal to Afghanistan, or the Mumeng dialect chain in Morobe Province, Papua New Guinea. Analysis of the results of intelligibility testing divides those chains into optimal chunks for the most effective coverage.

Use of Resources. Intelligibility surveys are the most efficient use of personnel, time, and other resources, both for surveys and as a basis for carrying out language programs. Some surveys have merely repeated earlier surveys by gathering longer word lists or by gathering word lists in additional places, but have still come out with an incomplete picture of the situation which could only then be clarified by testing intelligibility.

OBJECTIONS TO INTELLIGIBILITY TESTING

There have been various objections made to intelligibility testing besides those discussed by Casad (1974.57–61) and Simons (1979.13–16). Some of these are that it takes too long, it is too much work, it is too expensive because it requires two trips to each testing point, it is too subjective, there are too many variables, shortcuts can be taken, and the ends can be accomplished by other means.

In reply, it needs to be repeated that all other known methods try to get at intelligibility by measuring something else whose relationship to intelligibility is variable from dialect pair to dialect pair. The time, effort, and expense is less in the long run than carrying out unnecessary language programs.

Shortcuts have invariably led to inconclusive results either because some dialects were left out which should have been tested, or because the methods did not distinguish other abilities from intelligibility, such as reading, writing, translation ability, or second language ability.

There are variables in any kind of testing or measuring, and thus it is important to eliminate as many variables as possible in order to reduce the margin of error. It is important to be as consistent and thorough as possible, possibly reducing the number of dialects to be evaluated in the time available if necessary. The final proof of any

"... it takes too long, it is too much work"

survey is in the final outcome; does the literature produced in one dialect really reach adequately all the other dialects the survey predicted it would reach? Survey evaluations need to be ongoing as literature is produced and literacy and distribution programs are carried out. It may be that additional programs will be needed in dialects that were originally thought to be closer than they really are, or that other dialects can be covered by a single program that were originally thought to be more distinct than they really are.

In general, intelligibility testing has turned out to be fairly accurate. The main problems have come from setting the acceptable threshold of intelligibility too low (70% in Mexico) and not recognizing marginal intelligibility, from not screening out bilinguals from among the testees, from not reporting the standard deviation of scores and the number of subjects tested, from adjusting raw scores, and from not recognizing the importance of language attitudes that hindered materials from being accepted which were adequately understood (B. F. Grimes 1982.32, 1984a, 1985a.174, 1985b).

MORE ON INTELLIGIBILITY TESTING

Some additional understanding of intelligibility testing has been gained in recent years through surveys and the problems mentioned above. Other aspects of intelligibility testing need further emphasis because they have tended to be neglected.

Sample of Testees. Because testees have often not been adequately screened to weed out those that have had contact with the dialect to be tested, the sample tested has often included individuals who have learned some of the related dialect or language on top of inherent intelligibility (B. F. Grimes 1986a.19–20). Sankoff (1969.839–840) uses the term 'incipient bilingualism' to refer to bilingualism in a related dialect that does not include a great deal of learning. However, bilingualism can occur with dialects which have high inherent intelligibility and also with distinct languages which are related, in addition to bilingualism with a little learning all the way to a great deal of learning. I have used the term 'bilingual overlay' for this whole range of bilingualism in a related variety.

Bilingual individuals may be more eager to be tested, or may be put forward for testing by less bilingual individuals (M. Stubblefield, personal communication). However, bilingual proficiency has to be evaluated in different ways than inherent intelligibility; therefore the testing of those individuals has been incomplete as well as misleading,

because dialect closeness has little or nothing to do with how well individuals have learned the other dialect or language.

Taking a mean for true inherent intelligibility has validity because the spread of test results among speakers should be fairly uniform. Where bilinguals are part of the sample, however, the results are usually far from uniform. A mean which includes the results of bilingual individuals' scores causes the mean for inherent intelligibility to appear higher than it actually is for those speakers who have not had contact with the other dialect. If bilingual contact with another dialect were fairly homogeneous in a dialect community (Simons 1979.9), this possibility of variation would not be important in trying to determine understanding, because individuals' scores would be relatively uniform through bilingualism. However, in most parts of the world bilingual proficiency in a second language varies greatly within a community, so it is unwise to assume uniformity in a survey situation. For more accurate results it is best to screen out those who have had bilingual contact. If it is not possible to screen out such individuals, then the lowest scores will be the closest indication of what inherent intelligibility actually is for the monolingual population.

Because inherent intelligibility and bilingual comprehension are so different in the ways they are acquired and used, and especially because they need such different methods of evaluation (B. F. Grimes 1986a), it is helpful to use different terminology to distinguish the two kinds of understanding, as Sankoff does (1969.839–840), and in contrast to Simons (1979.2). For that reason I prefer to use the term 'intelligibility' only for inherent dialect understanding, and to use the term 'comprehension' for bilingual understanding. This terminological distinction helps clarify these important differences a surveyor must keep in mind.

Standard Deviation. The standard deviation of test results needs to be reported in addition to the mean and the number of persons tested. The standard deviation is the basis for showing statistically whether two groups can reasonably be treated as distinct on the basis of samples taken from each. The standard deviation can be an important clue to a bilingual overlay, especially if individuals have not been adequately screened for bilingualism. A standard deviation of 15% or more indicates the probable presence of a bilingual overlay on intelligibility. This does not mean, however, that a smaller standard deviation indicates that no bilingualism is involved.

Adjusting Test Scores. It had been suggested that test scores should be adjusted according to the hometown scores. If hometown scores were lower than 100%, it was thought to be because individuals might still be learning how to take the test during the hometown test, or during all the tests, or because of imperfections in the test itself (Casad 1974.31–32, Simons 1979.76–83, 174–179). The kind of adjustment chosen was either subjective (Simons 1979.174), or needed to be validated by additional data (Casad 1974.32).

J. Grimes (personal communication) points out that any error in the test is actually compounded by the adjustment. Rather than adjusting scores, he suggests that questions missed on the hometown test, which probably include ambiguous questions or questions about topics the text does not discuss, should be eliminated from the test, rather than continuing the confusion by stimulating guesses or causing discouragement. If only the questions which hometown testees answer correctly are used for further testing, then the result of the hometown test really is 100%, and the investigator can be fairly certain the test is valid. The testees have demonstrably learned how to take the test during the hometown test, thus eliminating the need to adjust scores. Care needs to be taken to formulate enough questions on the hometown test so that if some questions must be eliminated there will still be ten valid questions remaining. In order to do this, the questions should not be so simplified that a cognate, a loan word, or guessing alone can result in correct answers (C. Walton, R. Earl, personal communication).

Marginal Intelligibility. There is an area of marginal intelligibility which presents special problems for dialects whose closeness to each other is between that of adequate intelligibility and inadequate intelligibility (B. F. Grimes 1986a.20–21). This marginal intelligibility has surfaced in the cases of some Campa dialects of Peru (R. Rutter, personal communication), and the Rincon Zapoteco and Southern Rincon Zapoteco of Mexico (R. Earl, personal communication), where one dialect could understand narrative discourse in another dialect, but could not understand hortatory, expository, or explanatory discourse in that other dialect. Because intelligibility testing is usually done using narrative discourse, it is unable to distinguish this kind of marginal understanding. Simple narrative is even less useful in making this distinction. It is suggested that if intelligibility of narrative is between 75% and 90%, then further testing needs to be done using hortatory, expository, and explanatory discourse which includes more abstract and complex concepts than would be included

in narrative. Using 75% to 90% intelligibility as an indication of marginal intelligibility is based on comparisons carried out by Kirk in the Mazateco intelligibility testing survey in Mexico (Kirk 1970, Casad 1974.83–86).

Comparative Studies. Because it is now possible to use computers in comparing dialects by the comparative method, it is important to do that if comparative studies have not already been done in a dialect area. Intelligibility testing which does not have that foundation has sometimes not distinguished a bilingual overlay from dialect intelligibility (B. F. Grimes 1986a).

Although intelligibility between dialects is usually not equal in the two directions, a difference greater than 30% in the two directions probably indicates bilingualism in one direction. C. Walton (personal communication) reports that some Philippines languages may have as much as a 30% difference where there has not been contact. This does not mean that a smaller difference indicates no bilingualism.

One intelligibility test found 'intelligibility' between related varieties was 95% in one direction but in the other direction was 25% to 38%, depending on the dialect tested. It was concluded that these were dialects of a single language, because 95% intelligibility indicates adequate understanding. Comparative studies which had been done previously were overlooked, although they indicated the two were not dialects, but separate languages. The 95% understanding was from bilingualism in the sample of speakers tested, and not from inherent intelligibility.

In another survey, lexical similarity comparisons were not done, but 75% 'intelligibility' was reported for two languages that were in different linguistic groups too distantly related to have that much intelligibility. In addition, the 'intelligibility' was with a major lingua franca of the region, which should be considered an important clue to possible bilingualism.

In still another survey, some individuals from one town tested fairly high on one dialect, whereas others from the same town tested quite low on that dialect. Because the dialect being tested was the market center for the area, it is now clear that the higher scores were from individuals who knew the market dialect. Therefore, inherent intelligibility is really lower than the mean indicated.

Translation and Use of a Second Language. A test method that involves translating rather than answering questions in the vernacular (Simons 1979.9–10) is not able to distinguish between a person's ability to translate into a second language, his proficiency in that language, and his understanding of another dialect, which are three separate language skills on which individuals vary widely (B. F. Grimes 1986b.22). Even though this use of a second language enables an investigator to do intelligibility testing sooner than if he waited until he knew one of the dialects, and it requires less time and effort in setting up the tests, the results are less reliable because of the added variables of translation ability and knowledge of and interference from a second language. This kind of testing limits the sample of testees to people who are bilingual. In a case where the second language is related to the dialects being tested, it is possible that bilinguals may have higher comprehension of another dialect even if they have not had contact with it before, because the knowledge of a related language may enhance their understanding of a new dialect (Sankoff 1968.184, 1969.848, Simons 1979.114).

Group Testing. In general, testing a group together is less reliable than testing individuals, for several reasons. If bilinguals have not been screened out, then those bilinguals will understand more than others in the group, and they may give the impression that inherent intelligibility is greater than it really is. In face-saving societies, or societies where individuals tend to not disagree with their leaders, it may be difficult to determine if the spokesman for the group really reflects group understanding. For most parts of the world homogeneity in bilingual proficiency cannot be assumed; whether or not there is homogeneity can only be found out by sampling different age, sex, educational, and other relevant groups in each region. Even if bilinguals are eliminated from the sample, there are other uncontrolled variables in group testing. It is important to find out what individual understanding is, because in real life the individual will usually not have the benefit of group explanations of meanings. The 'highest group potential' (Simons 1979.15) may not be a realistic or useful indicator of intelligibility.

CONCLUSIONS

The testing methods described in Casad (1974) are the best general model for arriving at valid results for dialect intelligibility investigations in a variety of situations. Other methods and shortcuts leave too many unanswered questions, and need to be verified for

each pair of dialects separately by calibrating them against intelligibility testing anyway. Experience has demonstrated the importance of screening testees to eliminate those with bilingual proficiency in the dialects to be tested; of watching for marginal intelligibility; of reporting standard deviation, mean, and number of persons tested at each place; of not adjusting raw scores but rather eliminating faulty questions; and of making use of linguistic knowledge of the area based on the comparative method. The time and effort spent in carrying out these procedures is worth the resulting accurate picture of the situation that is gained.

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REVIEWS OF BOOKS

Women, fire and dangerous things: What categories reveal about the mind. **George Lakoff. Chicago: University of Chicago Press. 1987. Hardback. Pp. 614. $29.95.**

Lakoff begins by saying:

Many readers, I suspect, will take the title of this book as suggesting that women, fire and dangerous things have something in common—say, that women are fiery and dangerous. Most feminists I've mentioned it to have loved the title for that reason, though some have hated it for the same reason. But the chain of inference—from conjunction to categorization to commonality—is the norm. (p. 5)

Lakoff challenges the underlying assumptions of that 'norm', the classical view of categorization and the philosophical world view, objectivism. He states his purpose for writing the book in these words: *I began work on this book with the knowledge that objectivist views of the mind have a very wide currency in the academic world. Among my principal aims has been to characterize that view, name it, point out that it is an opinion, not a fundamental truth, and raise the question of its validity, so that it can be discussed in the open and no longer be presupposed automatically as part of an unquestioned background.* (p. 586)

In the first chapter he challenges the narrow view of science which considers as rigorous only hypotheses framed in first-order predicate calculus with a standard model-theoretic interpretation. He believes that rigor and precision must be characterized in a way other than predicate calculus because that view is not appropriate for the cognitive sciences since it assumes an *a priori* view of categorization, namely, the classical theory that categories are sets defined by common properties of objects. According to Lakoff, such an assumption makes it impossible to ask, as an empirical question, whether the classical view of categorization is correct. (p. 10)

"How did it come about that philosophers, linguists, and even many cognitive psychologists have come to view natural human languages in terms of formal syntax and formal semantics?"

Although Lakoff challenges the whole 'objectivist paradigm', he does acknowledge the common ground shared by objectivism and experientialism (his view). This common ground, he calls, BASIC

REALISM which consists of *(1) a commitment to the existence of a real world, (2) a link between human conceptual systems and other aspects of reality, (3) a conception of truth that is not based on internal coherence alone, (4) a commitment to the existence of stable knowledge of the external world, and (5) a rejection of the view that 'anything goes' — that any conceptual system is as good as any other.* (p. 158)

In his challenge to objectivist cognition, Lakoff states that the objectivist paradigm is not just about the world but is also about the mind and language. He believes that it is of the greatest importance to determine whether such objectivist foundations for cognitive science can be maintained in the face of empirical studies of natural human categories (prototype theory developed from these studies). He suggests that none of the objectivist doctrines concerning language and the mind can stand up to such scrutiny. (p. 196)

In a discussion of metaphysics and epistemology, Lakoff says that one of the cornerstones of the objectivist paradigm is the independence of metaphysics from epistemology. Objectivism holds that the world is as it is, independent of any concept, belief, or knowledge that people have. Minds, in other words, cannot create reality. He argues that this notion is false and is contradicted by just about everything known in cultural anthropology. He gives examples of 'institutions', e.g. government, which he believes are created by people, and are products of the human mind, and argues that therefore, people can create reality. He says cultural categories are real and they are made real by human action. He points out that trees and rocks may exist independently of the human mind, but governments do not. Therefore, in the case of social and cultural reality, epistemology precedes metaphysics, since human beings have the power to create social institutions and make them real by virtue of their actions. (pp. 207–208)

Lakoff also challenges the formalist enterprise and in the process challenges any model of language based on formalist mathematics, the concept of autonomous syntax and the formal system metaphor for grammar. He states:

Mathematical logic—one of the great marvels of twentieth-century intellectual life—has been used to justify an objectivist approach to cognitive science in general, and to linguistics and the philosophy of language in particular . . . The formalist enterprise in linguistics and in cognition generally is an attempt to impose formal syntax and formal semantics on the study of language and human reason in a particular way . . . Natural language comes with meaning, and when we normally reason using natural language, we reason about things in terms that

are meaningful; we don't just reason, and then find out what we were reasoning about and what our concepts meant. How did it come about that philosophers, linguists, and even many cognitive psychologists have come to view natural human languages in terms of formal syntax and formal semantics? (p. 226)

He makes the statement that generative linguistics defines a language as a set of strings of uninterpreted symbols, generated by some appropriately restricted version of production rules and then explains that such a definition of grammar is not a consequence of mathematical logic and not merely a value-free application of mathematics to natural language. It is instead, he claims, the imposition of a metaphor—a metaphor based on objectivist philosophy. The claim that syntax is independent of everything else is not an empirical result; it is a consequence of a metaphorical definition, which characterizes a commitment to study language according to that metaphor. (p. 227)

For every challenge, Lakoff offers arguments, claims and illustrative data to provide an alternative to objectivist views, which he believes preserves what is right about objectivism while permitting research into the nature of reason that goes beyond the limits that objectivism had placed on research. That alternative, for him, includes an experientialist paradigm, cognitive semantics and cognitive grammar.

In his attempt to develop this alternative, Lakoff brings together notions gleaned from the work of a diverse group of scholars. For example, he discusses Wittgenstein's ideas about family resemblance, centrality, and gradience; J. L. Austin's views on the relationships among meanings of words; Lounsbury's generative analysis of kinship categories; Berlin and Kay's research on color categories; also, Berlin's research on plant and animal naming; Roger Brown's study on children's learning of categories which has become known as 'basic' level categories'; Ekman's work on universal basic human emotions; and Rosch's work on prototypical categorization. (pp.12–57) Lakoff explains how the research of these scholars has influenced his thinking and played a significant role in the development of the paradigm he is presenting. Throughout the book he develops ideas drawn from this group of scholars as well as others.[1]

One of his claims is that the research done in prototypical categorization illustrates some general principles which he believes are at work in systems of human categorization. These principles are **(1) centrality**—basic members of a category are central; **(2) chaining**—complex categories are structured by chaining, i.e. central members are linked to other

members, which are linked to other members, etc.; **(3) experiential domains**—basic domains of experience which may be culture-specific can characterize links in category chains; **(4) idealized models of the world** (myths and beliefs included) can characterize links in category chains; **(5) specific knowledge overrides general knowledge**; **(6) the other**—conceptual systems can have an 'everything else' category; **(7) no common properties**—categories do not need to be defined on the basis of common properties; **(8) motivation**—the general principles may make sense of a language classification but may not predict exactly what the categories will be.

He claims that prototype effects have shown up in all areas of language—phonology, morphology, syntax and semantics and that in all cases they are inconsistent with the classical theory of categories and are in conflict with current orthodoxies in the field which assume the correctness of the classical theory. (pp. 60–61)

According to Lakoff, the main thesis of his book is that we organize our knowledge by means of structures called IDEALIZED COGNITIVE MODELS, or ICMS, and that category structures and prototype effects are by-products of that organization. His ideas about cognitive models were developed from four sources: Fillmore's frame semantics, Lakoff and Johnson's theory of metaphor and metonymy, Langacker's cognitive grammar, and Fauconnier's theory of mental spaces. (Chapter 4)

Metonymy, according to Lakoff, is one of the basic characteristics of cognition. People commonly take one well-understood or easy-to-perceive aspect of something and use it to stand either for the thing as a whole or for some other aspect or part of it. He feels that there are general principles which underlie the use of metonymy in languages and stresses the importance of finding which principles work for each language. Another interesting point is made in this chapter regarding the metonymic sources of prototype effects. This is the case in which a sub-category, member or submodel is used to comprehend a category as a whole and is used for drawing inferences or making judgments. He takes the concept 'mother' and illustrates how the concepts 'working mother', 'unwed mother', 'stepmother', 'adoptive mother', 'birth mother', etc. are cognitively related to a central case, where all the models converge. He believes that there is no general rule for generating kinds of mothers; instead they are culturally defined and have to be learned. (Chapter 5)

Lakoff uses the Dyirbal classification system[2] and the Japanese classifier 'hon' to argue that our conceptual system is dependent on, and intimately linked to, our physical and cultural experience and

therefore in opposition to the classical view that concepts are abstract and separate from human experience. He says that these two analyses suggest that such categorization is characterized by cognitive models of four types: (1) **propositional models** which specify elements, their properties, and the relations holding among them; **(2) image-schematic models** which specify schematic images; **(3) metaphoric models** which are mappings from a propositional or image-schematic model in one domain to a corresponding structure in another domain; **(4) metonymic models** which are models of one or more of the above types, together with a function from one element of the model to another. (Chapter 6)

The experientialist paradigm which Lakoff is promoting includes experiential realism, a version of internal realism. This is the version of basic realism which is set in opposition to the objectivist version. He uses the work of Putnam[3] who argued that an externalist perspective of realism is incoherent. According to Lakoff, Putnam argues that we cannot have a privileged correct description from an externalist perspective. We are not outside of reality. We are part of it, in it. What both scholars believe we need is an internalist perspective. This is a perspective that acknowledges that we are organisms functioning as part of reality and that it is impossible for us to ever stand outside it and take the stance of an observer with perfect knowledge. What is possible, for us, is knowledge of another kind: knowledge which includes the awarenesss that it is from a particular point of view, and knowledge which grants that other points of view can be legitimate. (pp. 260–261)

For an experiential realism, according to Lakoff, we need: *(1) alternative accounts of meaning, truth, knowledge, understanding, objectivity, and reason; (2) a theory of cognitive models capable of dealing with the facts of categorization and natural language semantics; (3) an account of relativism that avoids the problems of total relativism and makes sense of what stability there is in scientific knowledge.* He says that all of this must be done while maintaining basic realism, acknowledging: *(a) the reality of a world existing independent of human beings, (b) constraints on our conceptual systems due to the nature of that reality, (c) a conception of truth that goes beyond mere internal coherence, (d) a commitment to objectivity, and (e) an account of how scientific knowledge can be stable.* (pp. 265–266)

Lakoff claims that a philosophy of experiential realism requires a cognitive semantics and discusses the issues involved in a cognitive semantics theory: *(1) what makes concepts meaningful, (2) what is known about the nature of cognitive models, and (3) general approaches to meaning,*

understanding, truth, reason, knowledge and objectivity. (Chapter 17. He devoted Chapter 25 to a discussion of attacks and defenses of Model-Theoretic semantics, i.e. objectivist semantics.)

Linguistic theory based on an experientialist paradigm would result in cognitive grammar, in contradistinction to generative and other types of grammar. Cognitive grammar would require:

1. a cognitive semantics which makes use of propositional, metaphoric, and metonymic models, as well as the theory of mental spaces.
2. grammatical constructions which are direct pairings of parameters of form with parameters of meaning.
3. a concept of category which includes radial categories with prototypical centers, i.e. categories with central members which are related to other members through principled extensions
4. operating on the basis of a concept of motivation, rather than the dichotomy between predictability and arbitrariness.
5. the concept of 'ecological locations' within a grammatical system as a whole which are defined by central subcategories plus links of noncentral to central categories.
6. Syntactic categories and grammatical relations which are semantically and pragmatically motivated, rather than autonomous.
7. that most syntactic constraints be predicted on semantic grounds.
8. viewing grammatical constructions as holistic not atomistic, i.e. the meaning of the whole grammatical construction is motivated by the meanings of the parts, but is not computable from them.
9. that the lexicon and grammar be viewed as a continuum, rather than having a clear division.
10. that grammar not be viewed as independent of the rest of cognition. (pp. 464–465)

Three full chapters are devoted to detailed case studies which Lakoff uses as evidence for his challenges and claims.

The first case study is a presentation of an in-depth study of conventional English expressions which are used to talk about anger, e.g. He lost his cool. He's wrestling with his anger. He was foaming at the mouth. The study was done by Lakoff and Zoltan Kovecses. Lakoff says that emotions are often considered to be feelings alone, and as such they are viewed as being devoid of conceptual content. He argues that the opposite is true, i.e. emotions have an extremely complex conceptual structure, which gives rise to a wide variety of nontrivial inferences.

The Lakoff-Kovecses study reveals that there is a coherent conceptual organization underlying all these expressions and that much of it is metaphorical and metonymical in nature. Their primary analysis is based on the common folk theory of the physiological effects of

anger. They then go on to show principles of lexical elaborations, metaphorical entailments, and ontological correspondences. They also show that the metaphors and metonymies which express the anger concept converge on a prototypical cognitive model of anger, though not the only model, which also has a prototypical scenario associated with it.

They believe that their research shows that the expressions that indicate anger in American English are not a random collection but rather are structured in terms of an elaborate cognitive model that is implicit in the semantics of the language. (Chapter 1, Book II)

The second case study which Lakoff presents relates to the problem of polysemy. To begin the description of this case study, Lakoff says that the classical theory of categories does not do very well on the treatment of polysemy. The reason for this is that to have a single lexical item, the classical theory must treat all of the related senses of a word as having some abstract meaning in common—usually so abstract that it cannot distinguish among the cases and so devoid of real meaning that it is not recognizable as what people think of as the meaning of a word. On the other hand, he feels that protoypical uses of lexical items can be defined and then united with other data on natural categorization with the result that lexical items are viewed as constituting natural categories of senses. Nearly all of Chapter 2 of Part II is devoted to a detailed study and explanation of the polysemy of the preposition 'over' in English. He takes Claudia Brugman's work on 'over'[4] and extends it. Brugman's original study covered nearly one hundred kinds of uses of the word. The chapter is an elaborate presentation in which he uses 27 figures to graphically illustrate spatial schema which describe spatial senses and relations among them and secondly, which help to describe metaphorical extensions of the spatial senses. This chapter illustrates the importance of understanding meaning through the use of image schemas. Lakoff cites Eugene Casad's dissertation 'Cora locationals and structured imagery' as one of the most detailed descriptions of image schemas in a non-Indo-European language. (Casad claims that the structured imagery approach helped him to understand and explain the meaning and use of many Cora lexical items that had eluded classical type definitions.[5])

The third case study consists of a detailed explanation of 'there' constructions in English. Lakoff intends this study to be an illustration of cognitive grammar, his alternative to standard theories of grammar.

how a more elegant account could be given that covers the same range of syntactic, semantic, and pragmatic phenomena, while also showing the relationships among the constructions'. (p. 581)

For each of the three case studies, Lakoff does a superb job of showing what the implications of accepting his view of the data, interpretation, and explanation have for linguistic theory.

One of the most important contributions Lakoff has made with this book is to bring together ideas from a diverse group of scholars, philosophers, psychologists, anthropologists and linguists, to show cognitive patterns which must inevitably relate to language and meaning.

I did not agree with all of the generalizations and interpretations of data in this prodigious work (614 pages) but I deliberately chose not to focus on those disagreements in this review because the book is such a goldmine of creative and innovative ideas. It is the kind of book that I would beg, borrow or steal if I didn't have the money to buy it. (Good for me that the price of the book was writing this review. It was worth it!)[6]

E. LOU HOHULIN
Philippines

NOTES

[1]This book more fully develops seminal ideas which Lakoff presented in a monograph: 'Categories and cognitive models', *Berkeley cognitive science report* No. 2. UC-Berkeley: Berkeley Cognitive Science Program.

[2]The data from the Dyirbal language in Australia discussed in detail by Lakoff is taken from R. M. W. Dixon's research on the language. The title of the book came from the Dyirbal classification system.

[3]Lakoff discusses and develops ideas from Hilary Putnam's work. Although there are five books and papers written by Putnam in the bibliography, Lakoff, in his text, cites only his book *Reason, truth and history* and one paper 'Models and reality' which appeared in the *Journal of Symbolic Logic* 45:464–82.

[4]Brugman, Claudia. 1981. 'Story of OVER'. M.A. thesis, University of California, Berkeley. Available from the Indiana University Linguistics Club.

[5]Discussed by Casad during a presentation of data and ideas from his dissertation at a Forum, University of Oklahoma, Summer, 1987.

[6]See Hohulin, E. Lou. 1987. 'Concepts and categories: When is a tree not a tree?' *NOT* 122, for the implications of some of Lakoff's ideas for translation practice. (based on his Categories and Cognitive Models monograph) ■

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Coordinator's Corner

EUGENE LOOS

There is a certain amount of agreement in linguistic theory needed between field workers and linguistic consultants who aim to help field staff produce contributions to linguistic knowledge, and also between them and any editors who try to help find outlets for the descriptions or articles produced. It is interesting to note trends in SIL courses and the impact of those trends on researching consultants and editors.

In phonology, the *generative approach* prevails. In grammar, the schools vary but there is a focus on enabling the trainee to make a contribution unencumbered with formalisms. Here is a summary of the theoretical viewpoints that can be said to characterize each school:

TXSIL-UTA	□ **Tagmemics**
	□ **Stratification**
	□ **Typological-Functional** (UTA)
	□ **Government and Binding** (UTA)
SILUND	□ **Typological-Functional**
	□ **Relational Grammar**
OREGON	□ **Typological-Functional**
CANADA	□ **Tagmemic**, in transition to an as yet undefined viewpoint.
SILSP	□ **Structural** with a survey of theoretical viewpoints
HG	□ **Typological-Functional**, but with more focus on discourse than the Comrie-Shopen approach typically gives.
	□ **Semantics** a la Callow
HOLZHAUSEN	□ **Functional Grammar**, Dik type
CIL	□ **The French Model**
SINGAPORE	□ **Typological-Functional**

There is a stronger trend toward *typological-functional* grammar than I had realized. The Comrie and the Shopen volumes tend to be the ones used as texts. We can expect that as this trend makes itself felt, the descriptions that editors receive will be prose that attempts to give a clear idea of how the grammar of a language works. Such descriptions should have value as a contribution to knowledge well beyond the time that a specific theory could be expected to last.

As trainees go on to their master's degree or beyond they can expect to gain a competence in one or another theory. We are glad for articles that contribute toward our understanding of any theory, particularly those descriptions that clarify the insights that a theory offers about poorly understood linguistic puzzles. Equally welcome are articles that offer insights without requiring that the reader be versed in a particular theory. ■

4

How to Make Your Junior Partner an Equal Partner

FRANCIS L. GRALOW

My partner, Dottie Cook, started working with the Koreguaje people over five years before I did. Yet primarily because of her excellent job of orienting me, I was able to begin helping with translation during my second year of working with her and was carrying an equal load in all areas of your work by the end of the third year. I have observed other situations where one member of the team began first, or for some other reason was farther ahead than the other, and the one who was behind never attained the status of an 'equal partner'. This has bothered me. I wonder if the 'senior partner' did all she could to help the 'junior partner' catch up.

". . . make her orientation your first priority."

There are many advantages in making your junior partner into an equal partner. Once equality is attained, the work load for the senior is virtually cut in half. The responsibility, pressures and emotional burdens of the work can also be equally shared. I makes the junior more independent and productive and gives her more self-esteem and satisfaction in her work. If the junior partner never gets past being an assistant to her senior she never feels confident in the work nor is she trusted fully by the senior or the people they work with.

In order to bring your junior partner up to your level you will have to invest a lot of time at the beginning. It will mean a temporary slow-down in production, but it will be well worth it in the long run. You will be multiplying yourself rather than just acquiring a dependent helper. You must make her orientation your first priority. This does not mean you cannot work on other projects, but bringing her up to your level is of first importance and must take precedence over other things. You must be willing to put aside whatever you are working on to help her when she needs it. My partner did this for me for the first year—the length of time you feel you need to devote to this is dependent of your individual situation. You must attempt to teach her everything you know.

The first thing that needs to be taught is a basic introduction to the language and culture. My partner spent a month doing this for me

before we went to the language area for the first time together. I determined to work with the knowledge I had and not use the trade language, Spanish, at all. She had four lessons in how to learn Koreguaje done which I used for studying the language. This gave me enough background to be able to quickly get into some other areas. If you do not already have language lessons, it would be worthwhile to do a few to get her started. My partner also gave me text material to study and an orientation to all the language materials she had accumulated. By studying these and asking her questions I was able to learn things which I then made into lessons for myself. As soon as I understood another aspect of the language, I made a lesson on it with explanations and drills and studied it until I knew it well. These lessons were eventually published as a pedagogical grammar.

The 'Partners'
by Dottie Cook
Put those boxes over there, then get the broom, sweep the floor and after that get some water from the river...
I'm so glad I got a junior partner - they're so helpful!

While I was learning the language, Dottie gave me various unsolved linguistic problems to work on. As I found solutions to some of these problems I felt I was making a positive contribution in the early stages of my work, which is important for morale. She helped me get into conversation with the people by **(1) occasionally interpreting so I would know what was being discussed, (2) helping me formulate things I wanted to say to the people, and (3) translating what they said to me so I could respond.** *It is especially encouraging if you translate the nice things the people say about junior for her.*

As your junior is struggling with the language the people will naturally want to deal with you rather than her because they know

you better and because it is easier. This can be frustrating and discouraging for junior as well as unnecessarily time-consuming for you. You must ease the people into learning to relate to junior as well. There are several ways to do this. You can ask them to help you teach her and explain to them their responsibility in the matter. This will often help. Dottie did several things to help the people do this without directly asking them. One thing she did was to set up situations where they were forced to deal with me. She had established the practice of buying their artifacts for sale outside the area in order to expand their market. She taught me the necessary phrases and completely turned over the buying of artifacts to me. The people had to come to me if they wanted to sell their things. When it came to medical work she started out by having me do the simpler things that I could do without too much language ability. Then she worked me into taking half of it and simply being available when I ran into communication problems. She also sent me on errands, telling me where to go and what to say. These things were immensely satisfying when I was able to actually transact the business on my own. She gave me various responsibilities and left them with me, only giving me help when I asked for it. She also allowed me to stumblingly express myself to the people even when she was there, not interrupting, but sharing with me later if she wanted to correct something I said.

Just a few final words of advice. Don't be threatened by gifts your partner has that you may not have; rejoice in the additional abilities of your team. Be willing to relinquish the leadership and control so that the burden of it can be shared. Thoroughly explain the reasons for doing things the way you do, but be open to suggestions for improving on them. As junior learns the language and culture, give her credit for what she has learned and be willing to see things her way. Allow her to have insights about things you may have missed. Share your struggles and problems in the work with her and be open to her fresh outlook and help. *And finally, praise and encourage her.*

NOTES

Pike, Eunice. 1957. 'Partnerships: The junior-senior relationship.' *WBT Counseling Bulletin* No. 4. (This paper was given to us as we began our partnership and many good ideas were gleaned from it. Some of those ideas have been used in this paper.) ■

Categorized and Prioritized Bibliography on Language Typology

THOMAS E. PAYNE

The following is a list of references (mostly books) on linguistics (mostly syntax) that I would suggest all SIL language personnel have access to on the field. I have arranged the items in categories, and have ordered them within each category according to my impression of what their priority should be. In other words, if I could only take one book to the field it would be the first on the list, if two the first and second, etc.

The emphasis in this list is on **typologies of morphosyntactic constructions**. For example, Shopen's (1985) series contains several important overview studies such as Comrie on verb deriving morphology, Steve Anderson on inflectional morphology, Keenan on relative clauses, Longacre and Thompson on clause combining, etc. These articles outline the range of variation known to occur in the world's languages and give the fieldworker a benchmark against which to compare the facts of the language he or she is working with. These are the kinds of studies which have proven to be of particular value to field linguists in the throes of language analysis.

I have placed Givon's Syntax text first in the list of general works because it gives a brief overview of pretty much everything from the lexicon through simple verbal predicates, plus a good introduction to pragmatics, typology and functionalism in linguistics. However, it's weakness lies in that the theoretical portions of the book (primarily chapters 1 and 2) are difficult, and it stops just short of multi-clause phenomena. So this text does not really stand by itself, though under extenuating circumstances a determined fieldworker could extract much benefit from reading this book, and using it in the process of linguistic analysis.

Comments, suggestions and emendations to this list are encouraged. Note, however, that this is not an 'overview of the field', nor is it geared to the linguist who wishes to contribute to the development of some formal linguistic theory, or to couch a description in formal theoretical terms. Rather it is a prioritized list of items that will be of special value to field linguists who desire to write clear and insightful descriptions of individual languages.

General Studies

Givon, T. 1984. *Syntax: a functional typological introduction*. Philadelphia: John Benjamins.

Shopen, Timothy. 1985. *Language typology and linguistic description* (3 volumes). Cambridge: Cambridge University Press.

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Greenberg, Joseph. 1978. *Universals of human language* (4 volumes). Stanford: Stanford University Press.

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Haiman, John. 1983. Iconic and economic motivations. *Language* 59:781-819.

Lakoff, George, and M. Johnson. 1980. *Metaphors we live by*. Chicago: University of Chicago press.

Bickerton, Derick. 1981. *Roots of language*. Ann Arbor: Karoma.

Chafe, Wallace L. 1970. *Meaning and the structure of language*. Chicago: University of Chicago Press.

Studies of Particular Constructions, Patterns and Processes

Comrie, Bernard. 1978. *Aspect* (second printing). Cambridge: Cambridge University Press.

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Fillmore, Charles. 1968. 'The case for case'. *Universals in linguistic theory*, ed. by Bach, Emmon, and Robert Harms.1-88. New York: Holt, Rinehart and Winston Inc.

Fillmore, Charles. 1977. 'Topics in lexical semantics'. *Current issues in linguistic theory*, ed. by Roger Cole.76-138. Bloomington: Indiana University Press.

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Comrie, Bernard. 1978. *Ergativity. Syntactic typology: studies in the phenomenology of language*, ed. by Winfred P. Lehmann.329-394 Austin: University of Texas Press.

Hopper, Paul J., and Sandra A. Thompson. 1980. 'Transitivity in grammar and discourse'. *Language* 56:251-99.

DeLancey, Scott. 1981. An interpretation of split ergativity and related patterns. *Language* 57:626-57.

Haiman, John, and Pamela Munro (eds.). 1983. *Switch-reference and universal grammar*. Philadelphia: John Benjamins.

Hopper, Paul J. 1982. *Tense aspect: between semantics and pragmatics*. Philadelphia: John Benjamins.

Cole, Peter, and J. Sadock eds. 1977. *Syntax and semantics 8: grammatical relations*. New York: Academic press.

DeLancey, Scott. 1982. 'Aspect, transitivity, and viewpoint'. *Tense-aspect: between semantics and pragmatics*, ed. by Paul Hopper. 167-184. Philadelphia: John Benjamins.

DeLancey, Scott. 1985. 'On active typology and the nature of agentivity'. *Typology of language structures: nominatival, ergatival, activat*, ed. by F. Planck. 47-60. Berlin: Mouton.

Li, Charles N., ed. 1976. *Subject and topic*. New York: Academic Press.

Chafe, Wallace L, and Johanna Nichols, eds. 1985. *Evidentiality: the linguistic coding of epistemology*. Norwood, NJ: Ablex.

Nichols, Johanna. 1986. 'Head-marking and dependent-marking grammar'. *Language* 62.1:56-119.

Discourse — Pragmatics

Grimes, Joseph. 1975. *The thread of discourse*. The Hague: Mouton.

Chafe, Wallace, ed. 1980. *The pear stories: cognitive, cultural and linguistic aspects of narrative production*. Norwood, N. J.: Ablex

Givon, T. 1979. *Syntax and semantics 12: discourse and syntax*. New York: Academic Press.

Cole, P., and J. Morgan, eds. 1975. *Speech acts, syntax and semantics, vol. 3*. New York: Academic Press.

Phonology

Schane, Sanford A. 1973. *Generative phonology*. Englewood Cliffs: Prentice Hall.

Goldsmith, John. 1976. *Autosegmental Phonology*. Reproduced by the Indiana University Linguistics Club.

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10

Researching the Verb

URSULA WIESEMANN

The most complex research in any language is that of the verbal system. Currently I'm thinking of researching it under four different points of view: VOICE, ASPECT-MOOD, COMPLEX VERB PHRASES, and CLAUSE COMBINATIONS. By **VOICE** of the verb, one normally understands the transitivity parameters and the active-passive and ergative-anti-passive distinctions. I would like to class under this term all the changes in the verbal system that have to do with the participant structure and their cross referencing in the clause. So any time the number of possible participants changes, or the role structure changes in any way, I would class this as a change in voice. For details see the voice questionnaire.

Then there are the **ASPECT-MOOD** distinctions, which I believe are more basic than the tense distinctions. In addition there are languages without tense distinctions, where discourse information is contained in the verb. Even these languages seem to express the aspect-mood distinctions as outlined in the paper dealing with this part of the verb structure. I've not yet got a good 'handle' on the tense or discourse information distinctions that go beyond the aspect-mood distinctions. At the moment the only thing I know to do with them is to list them and try to describe their differences. I would try to separate the constructions into those made with the help of derivational affixes (or tone changes) and particles on the one hand and the presence of a second verb—the latter I would leave for study under the next heading.

The next two points of view concern the possible **COMBINATIONS OF TWO OR MORE VERBS** and the relationships that hold in such combinations. There is a basic difference between whether such a combination would function WITHIN A SINGLE CLAUSE—on phrase level—and whether the verbs concerned are predicates of a clause each. The problem is that it is often hard to distinguish between the two! On phrase level we probably have to do with such things as auxiliaries, combinations of modal and main verbs, or aspectual and main verbs, and the serial verb constructions. Which ones does the language have and how can they be defined? Any particles presumably have been dealt with under the previous heading—but who knows? Please see the relevant questionnaire for details.

The second part has to do with the COMBINATIONS OF CLAUSES into one sentence. Here the study of clause connectors is very important. Then there are all sorts of relationships that are expressed without the help of clause connectors. Which are they? And what signs are there to tell us what the relationship between the clauses is? A lot is expressed by combining in certain ways the forms isolated in the study of the aspect-mood distinctions; here we find out what types of combinations are possible and what communicative value they have. This section also has its own questionnaire.

These four topics, voice, aspect-mood (including tense and discourse information), verb phrases containing more than one verb and clause combinations, are to be the themes of four volumes on the structure of verbs. In subsequent articles in *NL*, I will treat each topic briefly. Any papers written on any one of the four themes I would be interested in editing and uniting—with other papers on the same theme—into a volume to be published by the Gunter Narr Verlag in Germany, who has agreed to do so. They published the pronoun volume which I edited and are pleased with the way it sells.

If you are motivated to write a paper or papers, please send me the draft(s) in two copies, one to SIL, B.P. 06-2552, Cotonou, Benin; the other to WBÜ, Postfach 603, 5909 Burbach 6, Germany. I will correspond with you promptly. Thank you! ■

SENTENCE REPETITION FOR
★★★★★ BILINGUALISM TESTING ★★★★★

The final version of the manual and field study report, *Sentence Repetition for Bilingualism Testing* (numbering approximately 80 pages, i.e. 40 sheets), is available for photocopy through the **South Asia Office, S.I.L. Horsleys Green, High Wycombe, BUCKS, England HP14 3XL**. This method for assessing second language proficiency has been developed by the South Asia Sociolinguistic Team in Pakistan. Quick to administer, the ***SRT*** is best suited as a screening test of proficiency in languages of wider communication, since the development process is rather involved. This report and manual will be published eventually, but for those who would like to investigate using this method, it is being made available now. *CARLA RADLOFF* ■

12

Comments on Analyzing Expository Discourse

SHERRI BRAINARD

0. Introduction

In 1985 I began studying expository discourse in Upper Tanudan Kalinga.[1] As I looked for discourse models to guide my analysis, I found most models were based on narrative texts and, in a few cases, hortatory texts. I did not find any models based upon expository discourse. Therefore, I chose a narrative model and attempted to use it to analyze some expository texts.

In doing so, I found that narrative models could not be applied directly to expository materials. Certain concepts in narrative models, such as BACKBONE and PEAK, were helpful, but specific features, such as backbone being composed of events, did not carry across to expository texts. In short, while narrative models provided a beginning point, in the end they did not explain what was most significant about expository discourse. Consequently, I found it necessary to adapt narrative models to fit expository texts. I also supplemented these models with other linguistic concepts, such as markedness and category suspension, that proved helpful in understanding expository discourse.

The following discussion is based on what I learned about analyzing expository discourse as I worked on Upper Tanudan Kalinga. The discussion consists of four parts. The first part gives a brief definition of expository discourse. The second part discusses a few points about charting texts. The third part describes the concepts that were helpful in analyzing expository discourse. The fourth part contains questions that proved productive in the analysis of expository discourse.

1. Definition of expository discourse

Expository discourse is defined here in terms of its surface structure rather than the speaker's communication purpose. I chose this approach because I wanted to be able to produce this type of discourse myself. Consequently, I needed to have a detailed understanding of the surface structure. Furthermore, as I analyzed expository discourse, it became apparent that the surface structure of expository discourse could represent several communication purposes.

For example, the speaker's intention could be to explain and to prove. In this case, there was little skewing between the surface structure and the underlying communication purpose. The speaker's intention could also be to influence or change ideas or behavior. In this case, there was greater skewing between the surface structure and the underlying communication purpose. In fact, such a text was actually a mitigated, or softened, exhortation. The surface structure of expository discourse in Upper Tanudan Kalinga then is a base expository form upon which an overlay of persuasive strategies can be placed. The overlay of persuasive strategies occurs when the speaker's intent is mainly to exhort.

2. Charting expository texts

In order to begin discourse analysis, it is necessary to have several well-edited expository texts in an interlinear format with a morpheme-by-morpheme or word-by-word gloss. If possible, paragraph breaks should be marked in each text. Paragraph breaks can be determined using Beekman and Callow's (1974) work on interpropositional relations. Normally a change in theme signals a new paragraph. Once the texts have been put into this format, they need to be charted.

". . . concepts . . . productive in discovering and describing what is most significant in expository discourse . . . : kinds of information, positions in sentences, markedness, and category suspension."

There are several methods of charting discourse information. Most of these methods, such as the Thurman or span chart, are based on kinds of information found in narrative texts. While these charts are useful as a starting point, they need to be adjusted for the analysis of expository discourse. For myself, I found Hale and Gieser's (1977) charting method the most helpful.Hale and Gieser suggest charting each sentence in a text according to its sentence positions, that is, prenuclear, nuclear, and postnuclear positions. They point out that there is a correlation between a sentence position and the function of the clauses and preposed elements that occur in it. This method is a simplified version of the Longacre-Levinsohn chart (1978).

Regardless of the method used, a number of points need to be kept in mind concerning charting. First, the linguist should work from the receptor language alone on the charts, if possible. In this way, more

of the text can be placed on a single page. This is particularly helpful for discourse analysis in which long spans of text are studied. Copies of the texts in their interlinear format should be kept close at hand for reference. For the purpose of charting, however, it is better to chart only the receptor language. Second, the linguist should not try to show everything on one chart. If he does, the chart soon becomes impossible to read. One way to avoid this is to make a basic chart, such as the Hale-Gieser chart, and xerox several copies of it. Then one or two kinds of information can be marked on each chart. All charts should be kept so that they can be referred to when comparing different kinds of information. Third, paragraph breaks and paragraph peaks should be noted on every chart. These two features are significant in the organization of information in expository discourse. Also, these features are often marked by distinctive surface structures.

3. Discourse concepts

When starting to analyze texts, it is helpful to refer to a general book on discourse analysis, such as Grimes' (1975) *The Thread of Discourse* or Kathleen Callow's (1974) *Discourse Considerations in Translating the Word of God*. Chapter headings, such as kinds of information, participants, cohesion, and topicalization or prominence, represent standard areas of investigation in discourse analysis. Any one of these areas can be a starting point for analyzing expository discourse. In addition, these books offer definitions and general introductions to basic features found in most discourse types.

In my own work, several concepts were particularly productive in discovering and describing what is most significant in expository discourse in Upper Tanudan Kalinga. These concepts are: kinds of information, positions in sentences, markedness, and category suspension.

The first useful concept is kinds of information. One of the first things a linguist should do is label each sentence in every text according to the kind of information it contains. Grimes (1971, 1975) discusses kinds of information found in narrative discourse. His list can serve as an etic list for other discourse types. It should be kept in mind, however, that Grimes' comments on the relative importance and function of each kind of information applies specifically to narrative discourse. While many kinds of information are common to both narrative and expository discourse, they differ in their importance and function, depending on the type of discourse in which they occur. For

example, in narrative discourse, events often make up the backbone of a text and are, therefore, important kinds of information. If events are deleted from a narrative discourse, the discourse falls apart. On the other hand, in expository discourse in Upper Tanudan Kalinga, events are mainly used as concrete examples illustrating abstract themes. In expository discourse, then, events are usually a less important kind of information. If events are deleted in an expository discourse, the discourse can still explain and prove its theme.

Simple sentences containing only one kind of information are fairly easy to label. Complex sentences containing more than one kind of information are more difficult to label. For complex sentences, it is necessary to note how the complex sentence relates to preceding and succeeding sentences. In respect to the surrounding sentences, one particular piece of information in the complex sentence should seem more significant than other information contained in the same sentence. If this approach fails, then one of the kinds of information in the sentence can be chosen as an arbitrary label for the sentence. The position of different kinds of information, such as theme, result, reason, and contrast, should also be noted. For example, in Upper Tanudan Kalinga, theme propositions always occur in the first or second sentence of a paragraph. Rhetorical questions which state negative evaluations come toward the end of a paragraph and signal the conclusion of the paragraph as well as performing other functions.

"Not all information can occur in prominence positions"

A second useful concept is the notion of sentence positions. A sentence can have three positions: prenuclear, nuclear, and postnuclear. Each of these positions typically contains certain information and grammatical structures, and performs specific functions. For example, in Upper Tanudan Kalinga, the prenuclear position is often manifested by preposed noun phrases that contain thematic information. The thematic information performs a cohesive function. By using the Hale-Gieser or Longacre-Levinsohn charts, the grammatical structures, such as clauses and preposed noun phrases, found in each position can be charted. These charts also help determine the kinds of information typically associated with each position and the function of that information.

Related to this concept is the idea that certain positions in a sentence are prominence positions. These prominence positions are often the first and last positions. Information placed in a prominence position in a sentence is foregrounded to signal that it is more important than other information in the sentence, or in some cases, the paragraph. Not all information can occur in prominence positions; therefore, it is important to note what kinds of information typically occur in prominence positions. In Upper Tanudan Kalinga, there are two prominence positions in a sentence: the first and last positions. Thematic information typically occurs in the first position, and results typically occur in the last position.

Once sentences in a text have been charted according to their prenuclear, nuclear, and postnuclear positions, the linguist can begin to look more closely at the grammatical structures found in each position. Clauses found in the nuclear position usually form the backbone of expository discourse. Note should be taken of the kinds of clauses that can occur in the nuclear position. Traditionally it has been said that in expository discourse, nuclear clauses are nonverbal clauses; however, in Upper Tanudan Kalinga, nuclear clauses are usually verbal clauses. This showed up clearly when sentence positions were charted and note was taken of the types of clauses found in the nuclear position.

The tense or aspect of verbs occurring in clauses in each sentence position can also be noted. Tense or aspect in nonnuclear clauses is often fixed, while tense or aspect in nuclear clauses is not. In regard to nuclear clauses, the tense or aspect that occurs most often in the nuclear clauses is the typical, or unmarked, form. If the majority of the nuclear clauses contain verbs in the nonpast tense, then the nonpast tense is the unmarked tense in expository discourse. If the majority of nuclear clauses contain verbs with nondurative aspect, then the nondurative aspect is the unmarked aspect. Deviations from these unmarked forms are regarded as marked forms. For example, if nonpast tense and nondurative aspect are the unmarked forms for verbs in nuclear clauses, and a verb in a nuclear clause occurs with past tense or durative aspect, then the past tense or durative aspect are marked forms. A third important concept, then, is markedness. Having singled out the unmarked form of such grammatical features as tense and

"The questions are . . . starting points for an investigation of discourse features."

aspect, the linguist should note any marked forms that occur. Distinguishing marked and unmarked forms can be a somewhat complex process. The linguist needs to follow a series of ordered steps to sort out marked forms and their functions. First, he needs to decide if the deviant form is a fixed form. If the deviant form can be predicted, then it is not a marked form. In Upper Tanudan Kalinga, verbs following the particle *nu* 'if' always occur in the imperfective aspect. On the other hand, verbs following the particle *dokadya* 'might' always occur in the perfective aspect. Because both forms are predictable, neither aspectual form is considered a marked form. Second, the linguist needs to decide if the deviant form is associated with its typical category and performing its usual function. If the speaker of a text describes past events that actually happened using past tense verbs, then the past tense of the verbs is performing its typical function of signaling past time. In this case, the past tense verbs are not marked forms. Third, if the deviant form cannot be explained in either of the preceding ways, then it is a marked form. In this case, the marked form is not associated with its typical category and is performing a different function. At this point, the linguist needs to determine its new function. The following paragraph suggests how this can be done.

A final concept is category suspension. Bache (1986) suggests that marked forms undergo what he calls category suspension. When this happens, the typical function of a grammatical feature, such as time for tense or aspect, and location for deictics, is suspended. The grammatical feature is assigned a new function (or distinction or contrast in Bache's terms). Some functions that are assigned to a grammatical feature once it has undergone category suspension are: foregrounding information, foreshadowing upcoming information, and presenting information as an assertion.

4. Questions to guide expository discourse analysis

The following questions provide guidelines for analyzing expository discourse. The questions are meant to be used as starting points for an investigation of discourse features.

Paragraphs

How does a paragraph begin?
What kind of information does it start with?
What types of sentences does it start with?

How does a paragraph end?
What kind of information does it end with?
What types of sentences does it end with?

Is there an order in the kinds of information that are presented?
Must a statement of the theme come first?
Can some other kind of information come first?
What kind of information can come next?

Do initial and final paragraphs follow the same pattern as noninitial paragraphs?
If initial and final paragraphs differ from other paragraphs, what are those differences? different kinds of information? different sentence types?

Prominence positions in sentences

Does a sentence have a prominence position? How many?

What kinds of information can occur in each position?

Theme

How is the global theme presented? The global theme states what the whole discourse is about.
Where is the global theme stated in a paragraph?
In what type of sentence is it presented?
Where is it stated in the sentence? preposed noun phrase? nuclear clause?

How is a paragraph theme presented? Does it differ from the introduction of the global theme? If so, how?

How many levels of theme are there?

Peak

Within a paragraph, is there a peak, or high point? The peak might be characterized by a unique sentence type, such as a complex sentence with several propositions. It might be characterized by certain kinds of information, such as results and reasons.

Does every paragraph have a peak? If not, which paragraphs are most likely not to have peaks?

Does one peak seem to dominate all the other peaks by relating to the global theme rather than a paragraph theme?

Other kinds of information

What other kinds of information seem crucial to effecting the speaker's intention to explain, prove, or persuade?

For example, contrast is important in expository discourse in Upper Tanudan Kalinga. Nothing is proven until it has been placed in contrast with something else.

Are there any distinctive grammatical structures? For example, a complex sentence with two or more propositions, or pseudo-cleft constructions.

Complex sentences with two or more propositions occur in Upper Tanudan Kalinga. The following example contains three propositions: *Whenever there were visitors, the man would invite them to eat because he wanted to show respect on behalf of the village*. This type of sentence is associated with theme statements and peaks of paragraphs.

Pseudo-cleft constructions also occur in Upper Tanudan Kalinga. The following sentences are examples:

What he wants is the book.
The book is what he wants.
The thing that he wants is the book.
The book is the thing that he wants.

Pseudo-cleft constructions perform two functions. First, they signal contrast as in: *What he wants is the book, not the paper*. Second, they signal exclusivity as in: *What he wants is the book and only the book — nothing else*.

Further discussions of pseudo-cleft constructions are found in Grimes (1975) and Prince (1978).

Participants

How are participants introduced and identified? preposed noun phrases? nuclear clauses?

How are participants referred to once they are introduced? pronominally?

What is the general person orientation for participants? first person? third person?

Is there any marked pronominal reference? What is its function?

An example of marked pronominal reference is when a participant has already been introduced and is eligible for

pronominal reference, but instead the speaker refers to the participant using a nominal or a name. In this case, the use of a nominal or a name is an example of marked pronominal reference.

How does the speaker refer to himself? first person singular? first person plural? third person?

How are the hearers addressed? directly? second person singular? second person plural? first person plural?

Cohesion

How is old information linked to new information?

What kind of information is tracked?

Not all information in a text is tracked; only that which is most important to the speaker's communication purpose.

What kind of cohesion occurs? lexical and grammatical cohesion? parallelism?

Are there any sentence conjunctions? If so, do conjunctions signal the same interpropositional relationships when they connect sentences as they do when they connect clauses within sentences?

In Upper Tanudan Kalinga, the conjunction *te* means 'because' when it links clauses in a sentence. As a clause conjunction, it always introduces a reason. When *te* functions as a sentence conjunction, however, it always introduces adversative information, never a reason.

Are different grammatical features used to track different kinds of information?

In Upper Tanudan Kalinga, conjunctions link reasons and results with theme statements, while repeated lexical items in preposed noun phrases provide thematic cohesion.

NOTES

[1]Upper Tanudan Kalinga is spoken by approximately 3,000 people who live at the southern end of Tanudan Valley. Tanudan Valley is located in the municipality of Tanudan in the province of Kalinga-Apayao, in northern Luzon in the Philippines. Upper Tanudan Kalinga belongs to the Central Cordilleran subgroup of Northern Philippine languages and is most closely related to Balangao, Bontoc, Kankanay, and Ifugao.

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- □ *Text*
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- □ *Proceedings of the Regional Meetings of the Chicago Linguistic Society*
- □ *Proceedings of the Annual Meetings of the Berkeley Linguistic Society* ■

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In Pursuit of Discourse Particles

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1. Longacre (1976) called them 'mystery particles and affixes', while Grimes (1975:93) dubbed them 'pesky little particles.' Both these descriptions aptly express the viewpoint of the linguist when he first runs into these queer little particles or affixes. I'm referring to the type of particle or affix that is found in many languages which defies a simple lexical gloss and in fact, generally elicits quite a range of unrelated meanings from a naive native speaker, and which at first appears to be arbitrarily 'salt and peppered' (expression borrowed also from Longacre (1976:468) throughout texts. If, however, the particle or affix is eliminated from texts, native speakers generally find the texts unnatural, which of course, leads the linguist to label it a 'discourse particle.' (In the context of this paper, I will use 'particle' to cover both particles and affixes.)

Deciding that it is a 'discourse particle' is only the beginning of providing an analytical explanation of its behavior in the language. How, then, does the linguist determine an analysis?

There is, of course, some preliminary preparation that will be helpful, such as reading a number of analyses of discourse particles in other languages. A list of relevant articles would include Binder 1977, Burgess 1979, Gratix 1978, Henriksen and Levinsohn 1977, Jones and Nellis 1979, Koontz and Anderson 1977, Moore 1977, Morton 1978, McArthur 1979, Salser and Salser 1977, and Wiebe 1977.

Furthermore, it is often helpful to chart the text. Various methods are detailed in the literature, such as in Grimes 1975, Jones 1977, and Longacre and Levinsohn 1978.

What I would like to focus on, however, in this paper are the goals which should guide the researcher as he pursues the analysis of a discourse particle. These goals provide a basis for measuring progress toward a satisfactory analysis. For illustration purposes, I draw on data from the Berik language, a Papuan language of Irian Jaya, Indonesia. These goals were followed in the analysis of the Berik particle ***ga***. My basic purpose here is to present the goals; a complete analysis of Berik ***ga*** is not detailed here, but is found in Westrum 1987.

2. The first goal is to *predict where the particle MAY occur, not where it will occur.* The key words here 'may' and 'will' correlate loosely with 'optional' and 'obligatory', respectively. It has been said that there are no truly optional elements in a text. Thus, the label 'optional' simply means the analysis has not yet been completed, because a thorough understanding of the 'optional' element often reveals that, in fact, it contributes in some important way to the structural coherence of the text.

". . . different approach to the notion OPTIONAL."

I would like to suggest a different approach to the notion OPTIONAL. If we view the construction of a text by a speaker or writer as having as one concern striking a balance between ambiguity and maximal clarity, we may regard an element as OPTIONAL if it:

(1) (a) Does not always occur where it may.
(Principle of ambiguity risk)

(b) May occur where strictly speaking it is not necessary.
(Principle of clarity)

English conjunctions such as ***then***, ***however***, and ***so*** fit this definition of optional. As an example, consider the following text. The words in parentheses may be deleted with little effect. These words *may* occur in the places indicated, but they need not (cf. 1a).

(2) Let me tell you what happened to Herb's son. He wanted to get coconuts. So he climbed a coconut tree. He climbed very high. (Then) he cut off some coconuts and they fell to the ground. Then he wanted to go back down. (Then) suddenly he fell.

In this same text, note the word ***so*** in the third sentence. While ***so*** may be deleted here, the result is not as smooth or natural. The same is true for ***then*** in the penultimate sentence. Strictly speaking, the conjunctions are not necessary at these points (cf. 1b). However, the text reads more naturally with them.

Discourse particles are often of this type. They are optional in the ways spelled out in (1). This is in contrast with obligatory elements, which may be defined as always occurring where they may. Examples of obligatory elements in English include subject of a clause, tense on the verb, and number marking on the subject.

The Berik particle *ga* is an excellent example of an optional discourse particle. In checking Berik texts with a native speaker, we found that it was quite acceptable to delete some of the occurrences of *ga*. Furthermore, it was acceptable to add *ga* in many places where it did not occur in the original texts. Thorough experimentation with adding and deleting *ga* led to fruitful hypotheses of how *ga* functions.

The following is the first paragraph of a rather lengthy text in Berik in which *ga* appears frequently. The *ga's* in parentheses may be acceptably deleted.[1]

(3) *1. Angtane bosna Usafe je GA tas tarnap ge nuin.*
person name Usafe he sago at.place DUAL live

2. Tesa GA belim taban, GA jes talebowel. 3. Ofona
sago cut.down finished it pounded pig

GA Jaume-mana, bosna GA Sebaf. 4. Ofo aiserem je
Jaume-POSSESSIVE name Sebaf pig this it

(GA) tesa GA jes tumawel. 5. Usafe GA wini naura
sago it ate Usafe woman two

gam tet. 6. Wi naurana aiserem je GA tesa
had married woman two this they sago

GA je ge talebawel. 7. Tesa gam ge wilni ofona
it DUAL pounded sago had DUAL pound pig

gam fortia tesa gam tumili GA jeber ge nuin.
would arrive sago would eat there DUAL stay

8. Jamare abaka tesa (GA) umef GA nanki fal.
until long.time sago remainder middle.part lies

9. Ofo Sebaf gamjon GA forial, tas nanki GA tuin.
pig Sebaf again arrived sago middle.part ate

'1. There was once a person named Usafe who lived near the sago acreages. 2. Whenever he finished cutting down a sago tree, he pounded it. 3. There was also a pig belonging to Jaume, whose name was Sebaf. 4. This pig always ate the sago. 5. Usafe had married two women. 6. These two women often pounded sago there. 7. Whenever they had pounded sago, the pig would arrive, and would eat the sago, then stay there with them. 8. This continued for a long time so that (only) a little sago remained in the middle. 9. Sebaf the pig then arrived, then ate the middle part of the sago trunk.'

The opposite situation is seen in (4). Here we have the first paragraph of a text in which *ga* is used more sparingly. However, it is

acceptable to add ***ga*** in all the places indicated with a circumflex (ˆ). (Note that a few of these are an either/or situation, where the ***ga*** may be added either before a particular word or after, but not in both places.)

(4) *1.* ***Ir*** ***ai*** ***jafnant*** ***Muarbumwer.*** *2.* ***Dewaf*** ***ai***
yesterday I went to.Muarbum for.matoa.nuts I

jafnant ***jamerai*** ***aiya*** ***forotfant*** ***Toganfu,*** ***Munanton***
went until I arrived Toganfu Munanton

3. ***Mutofo*** ***jeba*** ***ai*** ***(ˆ)*** ***fornant*** ***Habelem*** ***jinab***
at.Mutof there I arrived Habel's at.house

4. ***Dewana*** ***(ˆ or)*** ***ai*** ***(or ˆ)*** ***tombanant*** ***jamer*** ***taban.***
matoa.nuts I ate until finished

5. ***Wina*** ***gwanan*** ***gol*** ***aiserem*** ***je*** ***(ˆ or)*** ***fona*** ***(or ˆ)***
wife first married this she water

daktanant ***tesado.*** *6.* ***(ˆ)*** ***Aiya*** ***tumanant*** ***tiyon***
hung for.sago.pudding I ate sago.grubs

tul ***nanggal.***
fish tails

'1. Yesterday I went to Muarbum. 2. I went in order to get matoa nuts, until I arrived at the village of Toganfu on Munanton peninsula. 3. There at Mutof (peninsula, however) I arrived at Habel's house. 4. I ate matoa nuts until (I was) finished. 5. This first wife (Habel) had married, she hung water (over the fire) for sago pudding. 6. I ate sago grubs and fish tails.'

It is clear that ***ga*** is optional in the sense of (1) above. The goal, then, is to frame an analysis that will fit the occurrences of ***ga*** which actually do occur as well as those potential occurrences that are discovered through experimentation like that just described. However, it is NOT the goal of the analysis to predict where ***ga*** WILL occur; this is an impossible goal since ***ga*** is optional. The analysis only predicts where ***ga*** MAY occur.

3. In analyzing a discourse particle, one of the first steps is to note its distributional privileges. Many discourse particles are confined to a single slot in a structural unit, for example, as an affix to the verb or a particle that always follows the subject. Or perhaps it is a clitic that is not tied to a specific constituent but rather floats to a specific

position in the sentence (first and second position are most common). When a particle is associated with a specific constituent, this may give important clues to its function.

". . . distributional privileges."

There are times, however, when a particle may have more than one distributional possibility. Berik ***ga*** is this way: it may occur sentence-initially, following an NP (usually subject), preceding the verb, and even occasionally sentence-finally. (Because Berik is SOV, it is not always clear for a particular occurrence of ***ga*** whether it is to be considered as following an NP or preceding the verb, especially when the object is not present. This leads to a certain arbitrariness in the analysis.) *When a particle may occur in different environments, one should expect somewhat different meanings or functions in each different environment, but nonetheless one should seek a common thread of meaning or function.*

Analysis of Berik ***ga*** in its four different environments revealed the following functions (Westrum 1987):

Sentence-initially, as well as preceding a verb or verb phrase, ***ga*** indicates progression of time or logic. In a narrative, when events follow one another in chronological order, ***ga*** is used frequently in these positions. It simply indicates that the event tagged follows chronologically the last-mentioned event; it might be glossed 'and then.'

Following a subject or object noun phrase, ***ga*** indicates progression in topic. (By TOPIC is meant the roughly sentential or intersentential notion of 'what is being talked about.') It is very commonly used to signal a new topic, such as introducing an important new participant in a story, and also is commonly used for switching the spotlight back and forth between participants already on stage.

Sentence-final ***ga*** may occur when a subject or object NP is postposed to the end of a sentence, in which case it appears to be clarifying the identification of the topic. It may also tag a time word or phrase which occurs sentence-finally (normal position is near the beginning of the sentence). These latter type occurrences are relatively rare, but probably indicate progression on the time line. (The fact that these elements—the nominal or the time expression—are postposed

undoubtedly adds another dimension of meaning, which is not explored here.)

There are differences, then, in the functional meaning of ***ga*** depending on the environment in which it occurs. That is, the slot itself has a meaning component.[2] At the same time, however, there is a rather abstract meaning for the lexical item ***ga*** that can be deduced from the specific meanings in specific environments. There is a thread of meaning in common with all the different occurrences. The generalized meaning of ***ga*** might be expressed in this way: ***Ga*** signals progression in time (e.g. sequence of events), logic (e.g. ordering of ideas in logical sequence), and topic (e.g. succession of participant reference). It is a relatively low-level particle which marks a *succession* relationship between a pair of clauses (preceding and following). For example, the participant in the second clause or sentence succeeds the participant in the preceding one as being the topic ('what is being talked about') and/or the event described in the second one succeeds chronologically the event in the preceding one.

4. The most obvious task is to note where the discourse particle does occur. It is less obvious to study where it NEVER occurs. It is, however, equally important to *note the environments where a particle is never permitted*. This should include a *systematic examination of the restrictions in distribution, both syntactically (sentence-level and below) and contextually (discourse-level)*.

". . . a clue that the particle is associated with chronological movement . . ."

There may be certain types of discourse[3] where the particle is never found. For example, a particle may occur only in narratives or procedurals, but never in expository or hortatory texts. Such a situation might be a clue that the particle is associated with chronological movement or marking the eventline in some way. A further contextual restriction is the employment of the particle only in certain styles; compare usage of the particle in written vs oral texts, monolog vs. dialog, formal vs. informal, ritual vs. ordinary, polite vs. familiar, male vs. female.

On the lower levels (e.g. sentence, clause, phrase), there may be rigid syntactic restrictions. These might include restrictions regarding the co-occurrence of the particle in a clause with a certain tense or aspect marking, co-occurrence with a negative, co-occurrence with an

imperative or interrogative, and co-occurrence with certain other particles.

As an example, examining some of the restrictions governing the use of Berik ***ga***, we found that it occurred in virtually all discourse types (hortatory, expository, procedural, narrative, conversational), except in songs. It may occur in the sentence-initial position with only certain other conjunctions: ***ane ga***, ***jewer ga***, and ***ga enggam*** occur; however, ***jengga***, ***gamjon***, ***jeuga*** and ***jamer*** preclude use of ***ga***.[4]

" . . . precluded from being used in simultaneous constructions."

Further, ***ga*** has not been found in sentences containing sentential negatives. Nor is it found in sentences expressing simultaneous activities. Both these last two restrictions are naturally explained by the analysis: ***ga*** marks progression of topic, event, or logic. When a sentential negative is used, there is no progress, but merely a description of what did not happen. Further, because ***ga*** is used with chronological succession of events, it is precluded from being used in simultaneous constructions. In fact, both the negative and simultaneous restrictions could be predicted from the analysis, and the fact that they are supported by the data is good evidence for the credibility of the analysis.

The restrictions just described for Berik illustrate the usefulness of studying restrictions in distribution of a discourse particle. While some restrictions may seem arbitrary (such as co-occurrence with some but not all conjunctions), other restrictions are helpful in corroborating the analysis (such as the restrictions about simultaneity and negation).

5. It is sometimes supposed that the analysis is not good until it accounts for 100% of the occurrences of the particle in the data. But this is unrealistic. Certainly the analysis should account for at least 90% or more of the occurrences, but there may be many reasons why 100% accountability is not possible.[5]

The optional nature of many discourse particles makes it more difficult to formulate rules that take care of every instance. Differences in individual styles will be especially evident with optional particles, such as differences in frequency of use. Further, there may be a special extension of meaning that is almost idiomatic. A good

analysis will at least capture the central core of meaning, the range of most common usage. Finally, some uses of the particle may be of marginal acceptability, but that is difficult to assess in the average field situation. This is especially a problem with oral texts. (Working with edited texts or getting the judgment of a second speaker may help correct this problem.)

Even with extensively-studied topics such as tense or pronouns in English, the best analyses that have been produced to date always leave a residue of data that does not fit. For some reason, however, I have found that when a field worker begins studying discourse matters in a language, he is more troubled by pieces of data that don't fit neatly. At first, of course, this means that he should scrutinize his hypotheses more closely, try to refine them to make a better fit, or propose altogether new ones. But I believe that there comes a point when no better analysis can be found and yet there is still a small residue, and I believe it's time to submit the analysis to the scholarly world and let time and/or other linguists improve the analysis if they may.

NOTES

[1]*Gam* is also a discourse particle which is discussed in Westrum 1987. It signals 'out of sequence, regression on the time line,' much like a past perfect.

[2]Pike (1983:110) stresses that even slot has a meaning component, as indeed all form carries some meaning.

[3]See Longacre 1983, Chapter 1 for a good discussion of discourse types.

[4]Actually some of these appear to originate from a morphological combination with *ga*, which explains why another *ga* is not permitted.

[5]Perhaps even 90% is too high. Hopper (1979:221) suggests 80% in a relevant quote: It should be noted at this point that in discourse work explanations and hypotheses are not obviously validated with every example. Apparent inconsistencies and irregularities often mean that a certain proportion of the data contradict the general hypothesis. As a rule of thumb, I take this proportion to be about 20%; that is, I expect my explanations to account for an obviously large majority of the data. The remainder are then assumed not to be contradictory or arbitrary but to REFLECT A SPECIFIC INTENTION OF THE AUTHOR. The exegesis of this remainder may be quite convincing, or it may be guesswork.

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The Function of Glottal Stop in Gahuku

ELLIS W. DEIBLER, JR.

1. Introduction.
The purpose of this paper is to present evidence on the question of whether glottal stop in the **Gahuku** language[1] in Papua New Guinea should be considered a consonant or a feature of the syllable.

2. Phonemes of the Language.
There are five vowels: a, e, i ,o, and u. The only occurrence of long vowels as contrasting with short ones is in one-syllable words.

The consonants apart from the glottal stop are listed below in their phonetic form in the left column and in the practical orthography used up to this time (and in the rest of this paper) at the right.

b	r	g	v	l	g
h	s		h	s	
	z			z	
m	n		m	n	

Ignoring glottal stops for the moment, all syllables contain only a vowel or a diphthong preceded optionally by one consonant.

3. Is Glottal a Consonant?
In all our orientation to linguistics we were given the impression glottal stop was a consonant. In the phonetic charts there was a series of stops whose points of articulation were bilabial, alveolar, velar, and glottal; these functioned as consonants in syllable structure. I was not aware of other literature suggesting that glottal stop might function otherwise.[2]

By the time we arrived on the field and began analysis of the language a number of colleagues in other languages in the Eastern Highlands were also finding glottal stops, and the only question that was raised was not its status but how to symbolize it. In many Austronesian languages it had been symbolized by an **apostrophe**, and in Kate, which was structurally a highland language, **c** had been used, but *what was recommended to us most* was to use **q**.

Many years later Anne Cochran and I did some investigation of glottal stop. Our findings were incorporated into her M.A. thesis

which unfortunately to this date has not been published. In it she said:

> I want to suggest that glottal stop is not necessarily a consonant at all in the Papua New Guinea languages in which it has been analyzed as a phoneme . . . Its distributional irregularity made me question whether glottal stop is, in this case, acting on the phoneme level. Rather, I began to believe it is a feature of the syllable. Reaction from many speakers of highland languages in which glottal stop never occurred at the beginning of words and where it is the only non-nasal syllable closure agreed with this hypothesis.[3]

She states also that further testing among secondary and tertiary students and teachers gave the 'almost unanimous unsolicited opinion that glottal's function was to cut off the syllable so it was not acting like a consonant.' For an orthography they *preferred* the **acute accent** to the most widespread symbol for glottal stop, the **apostrophe**.

4. The Problem.

4.1. Theoretical.

As Cochran noted, in many of the languages we looked at, glottal had a distribution quite different from other phonemes. In **Gahuku** as in a number of other languages it occurred only syllable final. And in **Gahuku** it was the only consonant which could close a syllable. Furthermore, if it closed a syllable within a word the following syllable could begin only with **m**, **n**, or rarely, **v** or **z**.

4.2. Practical.

In ten years of conducting a literacy program in which glottal stop was symbolized by **q**, we never had any problem teaching it or having adult students reading it. Probably the main reason was that we used the syllable method, and we introduced **q** by saying that **naq** was a syllable which was pronounced differently from **na**, etc. But anyone who had learned to read in English or Pidgin first always had trouble with **q**. In writing, they would put it in where it didn't belong and leave it out where it did belong. And in reading they just plain objected to it. Our response was to ignore the criticisms and hope they would go away. We figured that the sound just didn't occur in English or Pidgin, so they didn't know what to do with it.

moluqne
molúne
molú ne
molu'ne
molu̲ne

5. Grammatical evidence.

There seemed to be considerable grammatical evidence that our interpretation of glottal as a consonant was correct. Grammatically it seemed to function as a segmental phoneme. First of all, a final glottal stop was usually lost before a suffix, and the initial suffix consonant was changed to a stop. This was accounted for by a SIMPLE MORPHONEMIC RULE. For example:

	gapo	gapoloq
	road	road-to
	gapo	gapoguq
	road	road-in
vs.	zohiq	zohitoq
	game	game-to
	zohiq	zohikuq
	game	game-in

So glottal was lost before the consonants /l/ and /g/, which then became their voiceless counterparts. This was describable by a simple morphophonemic rule.

Secondly, a set of possessive person-number suffixes seemed to clearly require glottal as a segmental phoneme:

	reg. pron.	reflex. pron.
1st sg.	neza	nezaqne
2nd sg.	geza	gezaka
3rd sg.	eza	ezaqa
1st pl.	leza	lezate
2nd pl.	lekeza	lekezatini
3rd pl.	keza	kezaqini

	'liver'	'older brother'
1st sg.	muluqne	uvoqneho
2nd sg.	muluka	uvokaho
3rd sg.	muluna	uvolaho
1st pl.	mulute	uvoteho
2nd pl.	mulutini	uvotipo
3rd pl.	muluqini	uvoqipo

If glottal is considered a segmental phoneme, morpheme segmentation is simple. If it is considered a modification of the vowel, a rule for suffixation becomes very complicated.

However, having duly symbolized glottal as **q** in all printed literature in the language, including the complete New Testament, in the late 70's we began to have second thoughts. Some people described the sound as *'where you pronounce it strongly'*. Others described it as *'where the vowel is cut short'*. In interviews with secondary and tertiary students, not only speakers of **Gahuku** but of other languages, after we gave them a few contrasting expressions to determine if they had that sound in their language, we would ask, *'Do you think this sound in language is a sound like **p**, **t**, **k** (i.e., a consonant) or do you think it is **a different kind of vowel**?'* And the answer almost invariable was, *'A different kind of vowel.'*

When I was working in the **Tokano** language (see footnote 1), having completed the SIL standard word list, and realizing the phonology (ignoring dialect variants) was identical that of **Gahuku**, I asked my informant how we were going to write glottal stop. He knew we used **q** in **Gahuku**, and I suggested half a dozed other possibilities—**q**, **c**, **double vowel**, **apostrophe**, **underlining the vowel**, **dieresis**, **an acute accent over the vowel**. After a minute he chose the *acute accent over the vowel*.

I had a typewriter with an acute accent as a dead key. So I explained, *'When you come to a vowel with that sound, you type the dead key first and then the vowel.'* That was of course backwards; phonetically the glottal closure comes at the end of the vowel. But I gave him a few words to try on, and within two minutes he had the idea, and after that typed the translation of 15% of the New Testament in **Tokano** and almost never made an error in the glottal stop.

Late in 1983, we were told that the **Gahuku** New Testament was sold out, but the people did not want a reprint—they wanted a revision. And the first change they wanted was to have the **q** removed. So, being in the United States, on a hunch I had a computer replace all **q**'s with an acute accent over the preceding vowel and then took printouts to PNG to get the reaction. Everyone was happy with it.

In my most recent visit to PNG, I asked people in many other languages how they perceived glottal stop. These included **Gadsup**, **Tairora**, **Agarabi**, **Benabena**, **Kamano**, **Gimi**, **Keiyagana**, and **Asaro**

in the Eastern Highlands, **Kate** in the Moroge Province, and **Nasioi** from Bougainville. Previously I also queried a speaker of **Kwara'ae** on Malaita in the Solomons. The answer almost invariably: 'I think that is a different kind of vowel.'

6. Conclusion.

In the case of glottal stop, the people's psychophonemic perception of it is more important for practical orthographical purposes than the grammatical evidence we had used.

It may well be that there are a number of languages where glottal stop functions as a consonant in some environments and as a feature of the syllable in others (evidence from some PNG languages would support this). If so, the orthographical solution may be different for the two environments within the same language.

If glottal stop is perceived as a different kind of vowel, every effort should be made to symbolize it in conjunction with the vowel (e.g., as an accent over the vowel), not by a device which would suggest it is a consonant.

And finally, a thorough investigation of glottal stop needs to be done for all languages in Papua New Guinea in which it occurs, and those findings made available.

NOTES

[1]The language, which surrounds Goroka in the Eastern Highlands Province, has always been called **Gahuku** by linguists, but Gahuku is only the name of the largest clan in the area, and people from other clans reject that name. There is no name for the language per se, but when people now wish to refer to it they use the word *alekano*, which means 'bring it' (the equivalent of *kisim i kam* in Pidgin). This contrasts with *tokano* and *daano*, the corresponding words with the same meaning and used to refer to the two languages immediately to the northwest. In future publications (including the revised New Testament and revisions of all existent literature in the language) the name **Aledano** will be used to refer to the language. This paper is based on field work in that language area between 1959 and 1979, as well as short periods in the area in 1984 and 1985. For a complete description of the language see Deibler, 'Semantic Relationships of Gahuku Verbs', *SIL Publications in Languages and Related Fields* No. 48, Univ. of Oklahoma (Norman) 1956.

[2]In his article 'Totonaco Phonemes' (*IJAL* 12:34) Herman Aschmann lists glottal stop as being 'intimately linked to the preceding vowel', and Kenneth Pike makes note of this in his 'Language in Relation to a Unified Theory of the Structure of Human Behavior (*Mouton, The Hague* 1967:387) but this never came to the attention of me or my colleagues in Papua New Guinea.

[3]Anne Cochran, 'Alphabet Design for Papua New Guinea Languages', submitted for M.A. degree to the University of Papua New Guinea, 1977, pp.45-46. ■

REVIEWS OF BOOKS

***Pronominal systems*. Edited by Ursula Wiesemann. Tübingen: Gunter Narr Verlag. 1986. Cloth. Pp. xxi, 501. 136 DM.**

Reviewed by RHONDA L. HARTELL

This book was a long time in coming but worth the wait. It is packed with a wealth of data and examples from around the world. I recall reading the questionnaire when it was decided my partner would write a paper on Lyélé pronouns and we have followed its progress ever since with great interest.

The preface opens with quotes from C.A. Ferguson and B. Comrie about the need for accessible, detailed data for comparative linguistics and studies in universals of language. The purpose of the book, therefore, is to provide just such data from a cross-section of languages around the world.

". . . packed with a wealth of data . . ."

There are three articles on Pacific languages, nine articles on African languages, five on Amerindian languages, and three more on topics of a more general nature. Knowing more of Africa, I appreciate that those articles are from widely diverse subgroups within two large African language families. Of general interest is the fact that all the languages in the book are either SOV or SVO with the exception of ***Ojibwe*** which has a free order.

The section on Pacific languages begins with ***Sursurunga*** which has a unique quadral number leading to a 19-form system. The pronouns are consequently the most varied and complex word class in that language. A study is done of the personal pronouns and some of their unique uses. The study of pronouns in ***To'abaita*** of the Solomon Islands covers general typological features, discourse and historical considerations. An annotated bibliography is included. The ***Berik*** article gives a basic description of the pronominal forms.

The nine articles on African pronominal systems make up half of the book. The languages are widely scattered across the continent and represent 2 major families and 5 subgroups. ***Gimira*** of Ethiopia, is compared with other languages of its family and pronomilization in discourse is described. The ***Chadic*** article is probably the most ambitious with 9 Chadic pronoun systems presented and compared.

The languages represent 3 of the 4 branches in Chadic. ***Yag Dii*** pronouns are set forth systematically with some time being spent on coreferential markings including logophoric markers. The ***Mundani*** article treats the relatively complex pronoun system of this Bantu language. Besides presenting the basic pronoun system, it goes on to treat emphasis and coreference. Another Bantu language, ***Konzime***, is discussed in light of the noun class system and referentiality. Moving to West Africa, ***Yaouré*** has a tendancy to fuse its pronouns with other morphemes. Various processes of fusion are presented. Within ***Lyélé*** of Burkina Faso, there are basic person and class distinctions for pronouns. These are described as well as various observations on their functions in discourse. ***Godié*** pronoun distinctions are described as well as a comparison given to the system of proto-Kru. Then details are given on the Godié pronouns in discourse. The final African article presents and compares pronominal systems in the subgroup of ***Western Oti/Volta***. These pronouns are discussed in terms of basic systems, roles, and the function of noun classes in the grammar and within text.

The third group of articles include Amerindian languages from both North and South America. ***Southwestern Ojibwe*** pronouns are presented in terms of the morphological and syntactic manifestation of their functions and categories. Bound and free pronominal forms are discussed in detail. The minimally contrastive person-gender-number distinctions within the ***Asheninca*** pronouns are described in the second article. The free and bound pronouns of ***Hallaga Quechua*** are discussed in the third article. The ergative case system of ***Maxakali*** is reflected in its pronominal system as well as number agreement between verbs and nouns. The final Amerindian article describes and compares several ***Macro-Jê*** pronoun systems. They are discussed in terms of person-number-gender, grammatical function, emphasis, and coreference.

The final section has three articles which speak in more general terms of children's pronouns, free pronouns, and grammaticalized coreference.

Pronominal systems was published from a computer printout and therefore the type style and mono-spacing make it somewhat difficult to read. Also, the binding doesn't look like it is going to hold up, especially in the hot climates where researchers likely to use this book are located. But apart from these minor problems, the data contained in the book should be quite useful to NL readers working on pronouns. ■

REPORTS

REPORT ON WALS CONGRESS

Reported by ROBERT CARLSON, *University of Oregon*

The congress this year (held the last week of March, 1988 in Niamey) was rather smaller than usual, since due to currency exchange problems not many could come from Nigeria and Ghana. The usual range of papers were presented, some good and some off the wall (including an exhortation to revive hieroglyphics for writing African languages). The paper presented by Rene Valette (SIL) on focus in Fulfulde was very interesting.

The organizing committee took the unfortunate decision to not distribute the papers till the last day of the conference (normally the full paper is distributed at the time of presentation—this is really a great help when you are listening to an accent you don't understand very well, perhaps in a language you barely know, since papers can be presented in either English or French), so there was not as much interaction as there might have been.

As so often happens, the personal contacts were the most profitable part of the congress. Kay Williamson from the University of Port Harcourt, Nigeria has been advocating the SOV hypothesis for proto-Niger-Congo for a long time, and provided me with some good additional ammunition to take with me to the conference on grammaticalization in Eugene in May (where Givon and Bernd Heine (Cologne) are going to disagree over my paper). Dramane Kone of the ILA is working principally on Mande languages (which interest me for their similarities to Senufo) and has begun the study of a Senufo language in CI (which interests me because my work so far has been on a Senufo language).

At past congresses there has been a very productive working group on Benue-Congo, but those of us who were in other branches of the family felt rather left out (there are never enough participants to form a group on Gur, e.g.). This time the group was expanded to include the whole family, and proposals were made for the future which hopefully will lead to extensive revisions of the classification as has happened within Benue-Congo in the past. Proposals were also made towards setting up a wordlist which will be more valid for lexicostatistics than the present hodge-podge. ■

ASIA AREA SURVEY CONFERENCE

Reported by EUGENE H. CASAD

The Asia Area Conference on Survey Data Collecting and Interpreting was held at the Green Valley Country Club of Baguio during October 12 13, 1987. I personally consider this conference tohave been of primary importance for the Asia Area's ongoing research and for the corporation's goals in the area of language surveys. In addition, for me it was a pleasurable broadening of my own exposure to the range of situations under which surveys are being carried out, as well as an enjoyable opportunity to interact with a number of my Asia Area SIL colleagues for the first time. Finally, I was also able to contribute somewhat to the conference, which is a satisfaction in itself.

I begin this report by summarizing the various papers presented in Baguio, and conclude with some implications of the conference for survey methodology.

The first day of the conference was devoted to the topic of bilingualism. Randy Kamp began with a paper titled 'Bilingualism Testing in the Philippines'. He detailed a comparison of the use of four test instruments during the survey of the Karao people of Benquet province. The test instruments employed included a proficiency interview, a self-score evaluation, a self-test questionnaire and a set of taped comprehension tests. The self-score test required subjects to rate their own ability in the languages that they spoke. This rating was in terms of a five-point scale. The self-test questionnaire consisted of a set of questions on the ways the subject claimed to be able to use a given language. These questions are grouped around a set of various levels of difficulty that are assumed to attach to particular tasks. The proficiency interview followed the procedures started in the SLOPE syllabus, a result of work by Barbara Grimes and others to adapt for SIL's use an Educational Testing Services approach to evaluating Bilingual proficiency. The taped comprehension tests included a 'hometown' test tape in Karao and two Ibaloi test tapes.

". . . he cannot always separate bilingualism from inherent intelligibility."

Kamp's conclusions are very significant. For one, he finds that he cannot always separate bilingualism from inherent intelligibility. He

also observed statistically significant correlations between self-score evaluations and interview scores and between the self-test questionnaire and the interviews. The strength and reliability of these correlations, however, is such as to require separate tests for intelligibility and bilingualism. Finally he concludes that narrative test comprehension tests probably test no higher than an FSI level 3.

". . . bilingual evaluation is not an exact science . . ."

In 'Surveying Language Proficiency', Steve Quakenbush describes two methods he used in a survey among the Agutaynon people of Palawan, Philippines. One instrument consisted of a self-reporting questionnaire. It required subjects to give 'yes' or 'no' responses to questions which are designed to sample language skills of varying difficulty. The other instrument was an oral proficiency interview modeled on the approach of the Foreign Service Institute and the Educational Testing Services. Quackenbush found a .56 correlation between the two methods, which compares favorably with the findings of other researchers, including Kamp.

Quackenbush appropriately concludes that bilingual evaluation is not an exact science and that proficiency data must be construed as an approximation of the true state of affairs. The use of one instrument or another depends on the purpose and extent of a survey. Quackenbush rightly prefers the direct test over the self-report interview, but wisely opts to retain both instruments for particular situations. Finally, he concludes that the FSI concept of levels of proficiency provides a useable, valid and meaningful standard for evaluating proficiency.

Since Barbara Grimes was unable to attend, I presented material from the SLOPE syllabus which was prepared by Barbara and others in cooperation with Dr. Thea C. Brun, head of the language testing unit of the Foreign Service Institute. This was elaborated at a workshop held in Dakkar, Senegal, in April of this year.

SLOPE, THE SECOND LANGUAGE ORAL PROFICIENCY EVALUATION, is based on the assumption that bilingualism is most accurately modeled as interactive behavior. This view largely determines the form, content and mode of scoring of the test.

The people involved in the test situation include the subject being evaluated on a language other than his mother tongue, a tester who speaks the evaluation language as his mother tongue, a language assistant who shares the mother tongue of the subject being tested and a linguist whois fully conversant with SLOPE, is certified to use it, and has at least a working knowledge of the language in which the subject is being tested.

"The test . . . may . . . not be perceived primarly as a test."

The test itself takes the form of a conversation basically between the subject and the tester and may thus often not be perceived primarily as a test. It is given in three parts: the warm-up consists of an informal interchange in which the tester and the subject talk about simple matters of every day life. The warm-up is designed to get the entire procedure off to a smooth start and to evaluate how well the subject can handle a routine conversation. Part two seeks to evaluate how well the subject can elicit information from the tester and then relate this information in his mother tongue to the language assistant. This part of the test not only samples the subject's comprehension of the evaluation language but also requires the use of a complex set of conversational skills. Part three requires the subject to present information of some kind to the tester. Here the subject's ability to form coherent discourse comes into play, as does his ability to field any questions that the tester might raise.

The subject's performance is scored against a grid which is stated in terms of a set of PERFORMANCE STANDARDS which are each linked to a set of five oral proficiency factors. The performance standards are labelled ***blocking***, ***dysfunctional***, ***intrusive***, ***acceptable***, ***successful*** and ***superior***. The ORAL PROFICIENCY FACTORS, then include ***comprehension***, ***discourse competence***, ***structural precision***, ***lexicalization*** and ***fluency***.

The actual evaluation is given in terms of six levels of proficiency that are defined by distinct constellations of characteristics. Finer-grained distinctions are achieved by allowing plus (+) values for performances that fall partly into two distinct levels. The levels are as follows:

0 = Subject is unable to function in the spoken language.

0+ = The subject is able to meet his immediate needs with phrases that he has memorized.

1 = Subject can meet his minimum requirement and carry on simple conversations about familiar topics.

2 = The subject can satisfy routine social demands and meet limited needs in other domains.

3 = Here the subject can speak the language with sufficient structural precision that he can handle most formal and informal situations.

4 = The subject is able to use the language fluently and accurately at all levels normally pertinent to his needs.

4+ = The subject's speaking proficiency is usually equivalent to that of a well-educated, highly articulate mother tongue speaker.

5 = The subject is functionally equivalent to a native speaker.

To summarize, SLOPE is a complex, but finely tuned instrument, whose reliable usage requires ample training for the tester, the linguist and the language assistant. It should prove to be a valuable tool for evaluating bilingualism. It is complex enough, however, that we would not want to have to employ it for every single language group in the world. There will certainly be no need to do so, either, as I will explain later.

The final paper on bilingualism was a paper by Carla Radloff reporting on ongoing research in developing a sentence repetition test for evaluating bilingual proficiency. This paper represents work by a team consisting of Radloff, David Marshall and Charles Meeker, and several Pakistani evaluators. The overall project displays a methodological soundness, concern for reliability and validity, and meticulousness second to none in the field.

The sentence repetition test consists of a set of 15 tape-recorded sentences which are played back to subjects individually through earphones. These sentences are determined beforehand to be maximally discriminating for particular levels of difficulty, and are not related semantically. The subject is expected to repeat each sentence verbally. He is scored as follows: *perfect score on a sentence = 3. One error on a sentence gives him a score of 2, and two errors on a sentence gives him a score of 1. Three or more errors result in a 0.* The test is said to be a correlated one which takes its meaning from its relation to an independent instrument such as SLOPE or some other evaluation (OE) measure. Its function is that of screening for people who are of different proficiency levels.

Radloff et. al. have taken special care to tie their work in with other kinds of research. In addition, they place heavy emphasis on training their testers in order to guarantee the reliability of their results. They found no significant differences between the way both Urdu and Pashto testers scored subjects. Neither the age of the subject nor his level of education had any significant effect on his scores. However, whether a subject had been introduced to the test administrator by an acquaintance or relative or had merely been encountered among bystanders did make a significant difference in the subject's scores.

Radloff et. al. point out that the Sentence Repetition Test appears not to distinguish between speakers who are at FSI levels of 3+ or higher in the evaluation language. This may well be an intrinsic limitation of the method, as the definitions of the FSI levels themselves suggest. I do not see this limitation as sufficient grounds for rejecting the entire approach; rather, the Sentence Repetition Test and SLOPE can be used to complement one another. In most cases, probably, a community will turn out to be below 3+ in the evaluation language. The SRT could pick this up readily. SLOPE could then be used for the cases in which the sentence repetition reports a very high level of proficiency.

The thing that really convinces me about the approach is the consistent and meticulous care the Radloff team has taken to do things properly. The steps they followed include, among other things, developing the test in the area where the evaluation language is spoken, selecting and training the personnel involved in the study, constructing and testing a long form of the test (40 50) sentences, modifying the long form into a short 15-sentence version, developing an index of the discriminatory power of the sentences and rating the participants by an external standard. The fact that the Sentence Repetition Test is easy to administer once it is developed is simply icing on the cake.

To summarize the first day of the seminar, we can aptly use Cal Rensch's metaphor that there are 'several windows on bilingualism.'

The second day of the seminar was devoted to several different topics. It began with a paper by Francis A. Gray of Far Eastern Broadcasting Corporation. Titled 'The Use of Language Data in Broadcast Research and Project Development,' it was an informative presentation of both the research being carried out by FEBC, the role played by information gathered by other organizations, the nature of

RICE (Radio In Church-planting Evangelism), the 'World By 2000' Declaration, and the scope of Christian broadcasting encompassed by Gray's research.

It is particularly interesting that the development of the Personal Computer was crucial to making Gray's project feasible. In addition, Gray has found the Ethnologue immensely useful and has benefited further from personal interaction with Joe and Barbara Grimes.

The last part of Gray's paper discussed briefly the organization of the research data and gives a listing by countries of the numbers of major languages that still lack Christian broadcasting. There are presently 154 known languages in this category, i.e. these languages each have more than one million speakers. Gray closes his paper with comments on the limitations of the data and their dependence on organizations like SIL for reliable information.

". . . survey as a multiple stage problem, . . ."

The next paper was by Mark Taber. His *Survey: A Picture of Maluku* was an informative presentation of what has been done in Maluku up to the present, the approaches employed in collecting survey data, the kinds of data collected thus far and a summary of their problems in training survey technicians. Essentially they view survey as a multiple stage problem, which is the realistic way to approach it. Their work so far has been on Stage One, the collecting of background linguistic and sociolinguistic data for the purpose of delineating the broad language groupings of Maluku. This stage is nearly complete. I would like to underscore the good job that Taber et. al. have done in carrying out their library research, collecting primary field data and relating all these data in a coherent and believable picture. This is in clear contrast to another survey project that I learned about at SIL-Norman last year and with respect to which I will comment later on in this paper. A glimpse of Taber's presentation show his use of both non-SIL sources (Blust 1977; 1981); Worm and Hattori 1981) and SIL sources (B.F. Grimes 1984; C. and B.D. Grimes 1984); Simons 1977; Gudschinsky 1956; Sanders 1977 and Smith 1984). By pulling together as a team over the last several years and making the most of all available resources, Taber et. al. now have a broad picture of the language situation in Maluku, and are in a fine position for training their teams further for teaching stage II in a reasonable manner.

The third section of Taber's paper discusses aspects of the situation they have faced including logistical problems, the presence of displaced language groups, the widespread occurrence of multilingual communities vis a vis official efforts to unify the country linguistically and, finally, the problem of how to measure and evaluate the implications of peoples' attitudes toward the various languages and language groups involved in contact situations.

In section 4, Taber outlines a further stage in the survey program. This includes the specifying in further detail of dialect interrelationships within larger groupings and the search for potential dialect centers by the use of intelligibility tests. Particular studies of bilingualism and language attitudes will complement the intelligibility testing. All these data, then will feed into the development of strategies for the allocation of personnel to given language areas.

Taber's repeated emphasis on team-work, and stages of the survey program are well taken and should be heeded in all corners of the corporation. Our trained personnel are limited in number and, furthermore, we are not all trained in the same thing. All this sounds suspiciously as though we complement each other and need to pull together over a long period of time if we are going to get the world's languages surveyed. In addition, a clear implication of Taber's presentation is that it will take a long time to do the job properly. Taber rightly points out that it is necessary for the investigator to have had extensive contact with the community in order to ensure that his data are valid. This is true for all kinds of data word lists, intelligibility tests, bilingualism tests, language use and attitude evaluation.

Interspersed throughout the second day of the conference were three short, but important presentations by Cal Rensch, which I summarize here as a single block. The first paper 'Calculating Lexical Similarity' mentions a few wrinkles in collecting word lists. This makes the point that the seemingly *easy* tool of taking a word list in a language one does not know is not so simple after all. Tony Naden, from his work inGhana, became so convinced of this that he wrote up approximately 150 pages of 'Lexinotes' specifically to help survey technicians do a good job in collecting word lists. One simple point is that phonologically similar words may be similar for several different reasons, i.e., they

". . . wrinkles in collecting word lists."

may either be related historically or they may all be related due to borrowing from one language to another.

Cal points out that for our purposes, we do not need to apply the rigorous procedures of historical linguistics to our data. Nevertheless, I suggest that, with the accessibility of the personal computer and the development of programs such as John Wimbisch's **Word Surv** and Don Frantz' **Compass G**, which Joe Grimes is incorporating into a menu-driven family of programs, it is becoming feasible to go after a description of historical patterns as part of our normal analysis of survey data. A couple of Asia area colleagues have mentioned that they do not ordinarily record the word lists on tape. I would like to suggest that they do so, especially in view of the tentative state of our investigators' knowledge of their languages. In addition, cassettes do not weigh that much or take that much space.

Interestingly enough, the Asia area survey teams have found that they need to take two different word lists from different individuals. This frequently shows up 5 percent to 10 percent of differences due simply to sampling error. The first step in calculating lexical similarity is to regularize the discrepancies. The actual calculation is based on applying a set of criteria to successive pairs of words. Rensch devised this set of criteria as a control for variability in researcher evaluations of lexical similarity. No hard and fast decisions are made with respect to words that fail to make the criterion. Such words could still be related historically. The purpose of the calculation at this preliminary stage of things is simply to identify the most obvious sound correspondences.

Rensch's second paper is called 'Sociolinguistic Community Profiles'. Here he addresses the problems we fall into in trying to evaluate characteristics which are not uniformly distributed throughout a population. His observation is that the members of a society that are educated and travel more often are more proficient in a given second language than their less fortunate fellow citizens. Cal would like to identify the specific factors related to this greater proficiency. Assuming that a multiplicity of factors underlies second language proficiency, Cal notes that the size of the sub-groups that are associated with these factors varies from factor to factor. His solution is to conduct a census in which a representative of each household is asked about a set of 20 categories outlined by Frank Blair in his *Sociolinguistics on a Shoestring*.

These data, then, can be used to construct a community profile based on age groups, sex, education level, etc. Profiles of different communities can then be compared, forming the basis for useful hypotheses and arriving at believable conclusions.

Rensch's final paper is a note called 'Language Proficiency.' Because multilingualism is so wide spread in Asia, and because extensive testing for bilingual proficiency is so time consuming and costly, the Asia area team is looking for diagnostic patterns of language use that would show that there is no need for extensive bilingualism testing. He offers two hypotheses, which they will be testing in the field. Extensive bilingualism testing will not be needed if:

(1) the second language is not used in the home/family domain and the children are not being raised as mother-tongue speakers of that second language.

or

(2) the second language has a standard dialect usually acquired through education and 80 percent of the population is not educated beyond four (successful) years of school.

The morning session ended with the presentation of a paper that I am still writing, titled 'Dialect Survey: Fifteen Years Later.' This paper is to be divided into three main sections. Section I cited the increased complexity in the survey task as we now perceive it. Some of us, at least, now see several areas as being interrelated to one degree or another: linguistic similarity, dialect intelligibility, bilingualism and language use (both between genetically related dialects and between unrelated languages), language attitudes, dialect extendability and the acceptability of vernacular literature.

". . . the increased complexity in the survey task as we now perceive it."

In addition, we are increasingly viewing survey as a set of stages of successive approximations to workable solutions. The initial stage of a survey consists of choosing an area for study, doing background literacy research and carrying out initial P.R. among educators, local administrators, etc. This is followed up by an initial trip into the area to collect word lists and certain kinds of sociolinguistic data. All the information gained during this stage of the survey allows us to make tentative major groupings of languages and dialects in the area under study.

The second stage consists of testing for intelligibility to discover potential dialect centers and the extendability of communication from given centers. Intelligibility testing is selected for dialects related by approximately 60 percent lexical similarity and above.

The third stage consists of testing for bilingual proficiency for those places where the degree of bilingualism is questionable. Finally, where bilingual proficiency may be adequate, the survey team will still need to evaluate attitudes.

Top of the head figures from Joe and Barbara Grimes are that possibly 50 percent of language assessment needs can be handled at Stage I, whereas another 30 percent can be handled at Stage II. Stage III, bilingualism testing, may be needed for another 15 percent of the cases, whereas attitude testing may prove crucial in as little as 5 percent of the cases. These figures are impressionistic, but we can begin to firm them up by checking survey results from Mexico, the Philippines and Sabah, at least.

Section II of my paper details the increased awareness of the characteristics of the relevant indicators of language assessment and the variables underlying them. To begin, linguistic similarity includes structured likenesses and differences at the syntactic, morphological, lexical and phonological levels. SIL language surveys have typically focused only on ***lexical similarity***, but we really need a broader measure of things. For example, syntactic patterns can be borrowed, and they definitely do change. At the phonological level, linguistic similarity is partly due to the operation of regular sound change, including splits, mergers, coalescences, and dissimulations. In addition, new segments frequently enter sound systems through borrowings, in many cases altering the set of phonemic contrasts in the indigenous sound system. Lexical similarity itself is partly due to historical relatedness; in part it also reflects borrowings from various sources. Older borrowings may be so well adapted to the native system as to be almost unrecognizable. Later borrowings are usually more easily recognized. In certain cases, individual lexical items can result from loan translations, or they may reflect marking reversals, as in the case of animal names in Amerindian languages. Morphological similarity can be affected by reanalyses of two sorts. In one case, a pair of distinct morphemes fuse to become a single one. In other cases,

" . . . awareness of the . . . relevant indicators . . ."

speakers impose a polymorphemic analysis on something that historically was either a single morpheme or had a different morphemic analysis. Morphemic processes such as the generalization of a particular morphemic shape throughout an entire paradigm affect linguistic similarity. Finally, morphophonemic processes of various kinds lead to a greater or lesser degree of lexical similarity among dialects.

Similar things can be said with respect to syntax. Syntactic patterns common to a pair of dialects may either reflect a common genetic base or they may be borrowed from one language or another. Commonly, sentence connectors and conjunctions are borrowed. The entire comparative construction has been borrowed from Spanish by a number of Mexican Indian languages. In addition, calques (loan translations) to some extent affect syntactic patterns. For example, Spanish ***Que Dios te lo pague*** = 'Thank you!' has appeared in Cora of Mexico as Che'e Dios timuaatanajchite'en and in Dos de Mayo Quechua as Dyosolpaki (Mann and Weber, 1979). In both cases, the indigenous phrase literally means 'May God repay you.'

I close this section with another pair of observations. First, linguistic similarity between a pair of dialects is typically symmetrical: i.e., Dialect A is as different from as B is from A. Second, linguistic similarity is partially grounded in societal phenomena, but usually this involves considerable time lag.

"The myth of MUTUAL INTELLIGIBILITY dies hard!"

The variables that determine intelligibility are largely distinct from those that determine linguistic similarity. In a nutshell, intelligibility is based partly on linguistic similarity between dialects, not just on lexical similarity. In addition, intelligibility is partly based on interdialectal learning, which I take to be a subclass of bilingualism (see Kamp, 1987). All the societal attitudes and situations that underlie bilingualism also feed into intelligibility. The time lag between societal contact and a discernable effect on intelligibility is quite distinct from the time lag associated with the impact of societal phenomena on linguistic similarity. Intelligibility is also typically asymmetric, i.e. speakers of dialect A understand speakers of dialect B better than speakers of dialect B understand those of A. (Grimes 1974, Casad 1974.) (Note: The myth of MUTUAL INTELLIGIBILITY dies hard!) In addition, intelligibility is normally distributed within a population, i.e. it is not

restricted to age groups, sex or educational level (see Kroeger, 1986). Nor is intelligibility restricted to particular domains of language use.

As for bilingualism, there are significant ways in which it differs from intelligibility. To begin, bilingual proficiency is not necessarily tied to the genetic status of the mother-tongue languages in question, whereas intelligibility is (see B.F. Grimes 1986). Bilingualism derives more directly from societal phenomena, including the kinds of interactions between the social groups in contact and the attitudes the mother-tongue speakers hold with respect to various factors. The purposes of such interactions also are relevant. Bilingual proficiency, therefore, is often restricted by age, sex, educational level and even the location of primary residence. It is often also restricted to particular cultural domains of language use. Finally, bilingual proficiency, like intelligibility, is often asymmetric. Thus the Kalagan of Northern Luzon learn to speak B'laan, but speakers of B'laan do not learn to speak Kalagan (Bus Dawson, personal communication).

Moving on to a consideration of attitudes, Roland Walker divides them into forces impinging upon the culture from the outside and forces arising from within the culture itself. External forces include language contact, government policies, immigration, economic forces and intermarriage. Internal forces include ethnic identity, cultural resiliency, traditional values, vernacular literature, language use and various other factors (Walker, 1986). Considerations such as the previous history of contact between the local culture and the outside one are also highly relevant.

Finally, the question of the acceptability of vernacular literature is a whole new ball game. Some of the factors that Walker mentions include the value of education to the community, community attitudes toward both the written and spoken languages, the differences between national language and vernacular language orthographies, the perceived ease of reading the vernacular, the amount of available vernacular literature and the community leaders' involvement with all aspects of the literacy program.

There are actually numerous variables in all of these sociolinguistic categories that I have not mentioned here, but what I hope to have done is to show clearly two things: that is some overlap among the sets of variables that underlie linguistic similarity, intelligibility, bilingual proficiency, language attitudes and the acceptability of vernacular literature. Basically, however, the sets of variables that

relate to these phenomena are distinct. Thus, one must use distinct measuring devices to properly evaluate them. The interrelationships are enough to allow an investigator to discover some correlations between certain indicators of select sociolinguistic phenomena. The differences are great enough, however, to ensure that no single device used for measuring one phenomena will allow one to predict with any acceptable degree of success to a distinct one. In other words, linguistic similarity is not an adequate single predictor of intelligibility (see P. Kroeger 1986; J. Grimes 1987), nor is an intelligibility test an adequate single predictor of bilingual proficiency and so on.

Section III of my paper will discuss our increased sophistication in the statistical treatment of survey data. Basically we have moved all the way from making simple point estimates of sample mean scores to testing for correlations by way of linear and multiple regression (c.f. G. Simons 1977; P. Kroeger 1986; J. Grimes 1986; R. Walker 1987; C. Radloff et. al. 1987).

We are beginning to see concern with the need to rely on multiple indicators of given sociolinguistic phenomena (c.f. Kamp 1987; Quackenbush 1987; Radloff et. al. 1987). This has been motivated by an increasing awareness of the complexity of the language assessment table and the need to validate our test instruments.

Too many **innovations** in survey techniques have been made without any concern for either reliability, i.e. consistency in measurement, or validity, i.e. the test really tells you what you want to know. The concern that the Asia Area survey teams show for those considerations by their first-class methodology is both a personal encouragement and an example for the rest of the corporation.

A classic **innovation** of the sort I am bothered by is seen in Simons' use of **group testing** in lieu of individual testing through earphones. He comments that he did not feel that it was right to exclude bystanders from the testing (1977:28). This is the only rationale he gives for making a fundamental change in a test design that has been successfully applied in Mexico, the Philippines, Sabah, India and Pakistan. My own feeling is that had Simons done his 'PR' more thoroughly he would have found no major obstacles to the testing.

There are several problems in the group testing method. For one, it often turns out that a single person in the group is a particularly strong personality and succeeds in imposing his own will on the

whole. How his actual understanding relates to the true population value is anybody's guess. On the other hand, one or two members of the group may understand the dialect being tested much better than the other group members. In this case the score would be skewed higher with respect to the population value. In a third case, the members of the group could pool their knowledge to give a score that would also be skewed high. A general weakness of all of this is that the investigator is often going to be left with a score based on a sample size of one, rather than the sample size of ten that he would have had if he had tested individually. I am not satisfied that Simons' controls for these are sufficient (Simons 1977).

Simons also changed the method of scoring the intelligibility test from a percentage scale to an interval scale of '1' to '4'. He did this claiming that 'the results of methods which yield percentage scores are already too precise for the level of statistical significance that can be attached to them' (1979:38). As it stands, the statement is misleading. On that basis, however, he switches to an interval scale such that **3 = Full Intelligibility, 2 = Partial Intelligibility, 1 = Sporadic Recognition** and **0 = No Understanding** (1979:10). Although interval scales are widely used, Simon's switch at this juncture auto-matically means that we cannot compare statistically his results with those deriving from more standard approaches (cf. P. Kroeger 1986:319).

A number of these non-validated versions of intelligibility tests have come about from good intentions people were concerned about the presence of error in our survey results. So they tried to design an error-free instrument. All such efforts are bound to fail, however. The best we can do is control for whatever disturbing factors are in the situation and then try to estimate how much our error is. As Joe Grimes put it to me, the role of methodology is as follows:

(a) Good methodology makes you aware of experimental error;
(b) It helps you to estimate the size of the error, and
(c) It tells you how much a given error hurts you.

The rest of my paper, which I do not summarize here, describes briefly the array of evaluative instruments being put at our disposal and the powerful computational tools that are being developed to help us catalogue, describe and analyze our survey data.

The Tuesday afternoon session was devoted to two papers on language attitudes. The first 'Language Attitude Test in a Multilingual Setting' was presented by Ronald Krueger. The test was

developed as a course requirement for SIL **4340-1 Sociolinguistic Surveys**, which was taught by Joe and Barbara Grimes at the Oklahoma SIL School.

The test that Ron and Joanne designed was intended to sample several areas of interest. These included the domains in which Hindi, Urdu, Gujerati and English were used, the propriety of the usage of each of these languages in the presence of non-speakers, the attitudes of people toward each of the languages in question and their attitude toward the peoples who spoke these languages.

Krueger made a few quite significant generalizations from his study. For one, language attitudes do not have just simple plus and minus values, but rather reflect a whole range of strengths and meanings. Furthermore, attitudes are not uniform throughout a culture. This means that one must sample carefully and thoroughly if he is going to draw valid conclusions. Finally, you can build controls into an attitude test. Ron and Joanne did this by including two forms of each question at different points in the test. This allowed them to judge the consistency of the subjects' responses. The test consisted of 30 questions, some of which had several parts. Finally, it took about one and a half hours to administer. Although Kreuger did not suggest his test as a model for others, I have already used it this way in the Norman survey course.

Kreuger's design contrasts clearly with the project that I referred to earlier. This proposed attitude test consisted of twelve questions and was supposed to take only 10 or 15 minutes per subject. The design mentioned earlier survey work in the area but showed no intention of relating the attitude study to that earlier work. In addition, the principle investigator would have been an outsider to the cultures he was evaluating. This concerns me greatly.

Regarding patterns of language attitudes, Rensch mentioned that in the Asia area surveys they noticed two. The first pattern is based on the idea of 'limited good.' In this view, people tend to think that if one of the various languages they speak is good, then the other is bad. The second pattern is based on the idea of 'total good.' All the languages in the multilingual setting are held to be good; one serves for one purpose, the other serves for another. Needless to say, these two situations involve two rather different strategies for establishing and carrying out field programs.

The last paper in the conference was Roland Walker's 'Towards a Model For Predicting The Acceptance of Vernacular Literacy by Minority-Language Groups.' Walker's task is to identify those sociolinguistic variables that best predict that a given vernacular language group will accept literacy materials published in the mother-tongue. Walker assumes that attitudes toward language underlie the reason why a society accepts vernacular literacy materials. He also notes that it is difficult to measure attitudes directly. In this he implicitly agrees with Quackenbush who notes that we cannot measure attitudes, we can only infer them (1987:1). Walker goes on to assert that sociolinguistic forces work together in a community to determine language attitudes (1987:5). Since these are more readily assessible, Walker chooses his sets of predictor and criterion variables from these and devises ways to quantify them. In particular, he identifies several factors.

He selects a set of criterion variables which represent three aspects of the acceptance of vernacular literature and which are linked to Cooper's SCALE OF PERVASIVENESS. He also selects a large set of predictor variables, which he sub-divides into program variables, sociolinguistic variables and orthographic variables. Walker's criterion variables include the volume of purchase of vernacular literature, reading ability in either the national language or the vernacular, reading in informal settings and language usage in the church (pp. 10 11).

Walker gives the following list as his set of predictor (=independent?) variables, (pp. 18 25):

1. Distance from a town.
2. Intermarriage with non-VH speakers.
3. Presence of non-natives in the community.
4. Proficiency in national language.
5. Leaving Community to find employment.
6. Importance of National Language to economic advancement.
7. Need National Language proficiency for present job.
8. Attitude of local officials to vernacular literature.
9. Official policy of national or state government to vernacular literature usage.
10. Ethnic identity of people in the community.
11. Language use: in home.
12. Language use: in community.
13. Language use: in church or religion.

14. Language use: in occupation.
15. Language use: in school classroom.
16. Language use: in government offices.
17. Language use: in singing.
18. Language shift: change in usage patterns.
19. Percentage of population living by the Bible.
20. Relative size of the language groups.
21. Population of the community.
22. Degree of formal education.
23. Value of education to the community.
24. Previously using national language Scriptures.
25. Percentage of those reading the national language first, but also reading the vernacular.
26. Attitude of community leaders to own vernacular literacy.
27. Policies of the churches toward vernacular literacy.
28. Community attitudes toward spoken languages.
29. Community attitudes towards written languages, orthographic variables.
30. Difference between vernacular language and national language orthographies.
31. Ease of reading vernacular *vis-à-vis* reading the national language.

PROGRAM VARIABLES

32. Amount of vernacular literature available in the community.
33. Acceptance of innovators.
34. Readability of translated Scriptures.
35. Readaoility of secular vernacular literature.
36. Extent of vernacular language promoter's early contacts with the community.
37. Community leaders' involvement in orthography decisions.
38. Community leaders' involvement in other aspects of the vernacular language program.

His task is first to calculate the correlations between each predictor variable and each criterion variable. Then he sorts out the statistically significant correlations from the non-significant ones. Finally, he calculates multiple regression equations based on the correlations between given sets of predictor variables and each of the criterion variables. For criterion one, *the purchase of vernacular literature*, he combines in a single regression equation community involvement, language use in the school, the importance of national language proficiency, the population of the community, the need of the national language for employment, the percentage living by the Bible, the general level of education, the percentage of people previously

reading the national language and language used in the church. All this gives him a multiple correlation coefficient of .806, which in turn has a variance of .65. In other words, there is sufficient complexity involved in just the single criterion of purchase of vernacular literature that with nine correlated variables Walker can still only account for two-thirds of the variation in the measurements. For criterion four, use of vernacular in churches, Walker comes out a little better, with a multiple correlation of .840, based on six predictor variables. This accounts for about 70 percent of the variation in Walker's measurements. The relevant predictor variables were language use in the churches, ease of reading the vernacular language, the proportion of non-natives in the community, the need of the national language for occupation, the degree of difference between national language and vernacular language orthographies and the percentage of vernacular readers who previously read the national language.

For criterion two *reading ability* (in both languages), the multiple correlation turns out to be .679. This is based on the factors of intermarriage within the community, community involvement in the orthography and degree of orthographic difference between the material and vernacular languages. The variance is .461. In short, the above factors taken together account for some 45 percent of the variation in the measurement of these variables. For some reason, Walker gives no discussion of the variables that relate to criterion three, i.e., *the informal usage of the languages*.

"The complexity is typical, but still mind-boggling."

Walker concludes that 'there are significant relationships between the measures of vernacular language literacy acceptance and the same sociolinguistic forces that are related to language shift and language death (p. 58).' Somehow I think that is misleading. I doubt that language shift is the same phenomenon as language death. That some of the factors that relate to language shift would also be relevant to the acceptance of vernacular literature is not completely surprising. On the other hand, I do not think that literacy is linked to language death. It is perfectly clear from reading Walker that he has not demonstrated that literacy and language death are related. Furthermore, I hesitate to think of the use that SIL detractors could make upon receiving news of Walker's conclusion that literacy and language death are related. I would like to see Roland make his claims more realistic.

Essentially, Walker's paper represents an interesting, and necessary exploratory attempt to specify the sociolinguistic variables that relate to various aspects of language attitudes and the acceptance of vernacular literature. The complexity is typical, but still mind-boggling. Whereas most investigators attempt to study only two or three variables at a time, Roland has tackled thirty-eight! There are many particulars at which I could quibble, but, mostly, I would like to commend Roland for his hard work, and I encourage him to keep at it.

I would like to express a couple of reservations about the use of attitude data, however. First, data on language attitudes should never be the sole, or even, primary, basis for deciding where to establish literacy programs. On the other hand, such data may be extremely important for determining how to carry out a project once it is established in a given location.

In summary, the papers presented at the conference relate to important topics and reflect careful work on the part of well-trained people. There are several implications to be drawn from all this. Teamwork is one of the most salient: dedicated people working toward a common goal. Sound methodology there is a pleasing and consistent concerted effort to develop reliable test instruments and validate them. These people are using fairly sophisticated statistical methods, but, for the most part, know when to opt for their intuitions instead of being misled by the numbers. *Realism*—they recognize the complexity of the task and deem it doable, but also know that it cannot be done overnight or even in three or four years. *Continuity*—rather than throw overboard everything else that previous researchers have done because of their exhilaration over the development of new research in the areas of bilingualism, language use and attitudes, they have taken the best from comparative work, lexicostatistics and intelligibility testing and have melded it into a coherent program. In short, these people have not only validated what Crawford, Bradley, Kirk, Stoltzfus and Grimes developed in Mexico, but they have gone beyond that, teaching us new and useful things.

In closing, we are considering incorporating into a single volume the papers given at the Baguio Seminar. In this way we will be able to put into the hands of other people elsewhere in the corporation the heart of the good that is presently coming from the Asia area.

Beyond this possible volume, I need to mention the fine paper written by Paul Kroeger, called 'Intellegibility Patterns in Sabah and the Problem of Prediction' (1986), which was published in *FOCAL I: Papers from the Fourth International Conference on Austronesian Linguistics:* 309 339; Paul Geraghty, Lois Carrington and S.A. Worm, eds. This paper represents the finest statistical treatment of survey data that I have seen to date. Finally, Frank Blair has written a preliminary version of a survey handbook called *Sociolinguistics on a Shoestring*. It includes six chapters. Chapter I is introductory, giving definitions and outlining the scope of the work. Chapter Two is called 'Survey Planning'; Chapter Three covers 'Dialect Areas'; Chapter Four discusses 'Sampling.' Chapter Five covers 'Bilingualism' and Chapter Six treats 'Language Use and Language Attitudes.' This is a survey handbook in the full sense of the word and could easily be used in countries outside of the Asia area. ■

☛ SYSTEMIC LINGUISTICS ☚

The **Tenth Finnish Summer School of Linguistics** will be organized at the University of Jyväskylä from June 5–9, 1989. The subject of the summer school will be SYSTEMIC LINGUISTICS The participant fee will be about 400–500 FIM. For further information contact *Salme Vaaraniemi or Kari Sajavaara (Department of English, University of Jyväskylä, SF-40100 Jyväskylä, Finland, tel. +358 41 291 620).* This summer school will be followed by the 16th International Systemic Congress.

The 1989 **16th International Systemic Congress** will be held by the University of Helsinki, Helsinki, Hansaari, Finland from June 12–16, 1989. The Congress will be scheduled in two parts: 1) Monday–Wednesday: for plenary, section, and poster-session papers; 2) Thursday–Friday: devoted to bridging workshops. For more information and for preregistration forms write: *16-ISC 1989, Dr. Eija Ventola, Language Centre, University of Helsinki, Fabianinikatu 26, SF-00100 Helsinki, Finland.* The deadline for all proposals and also preregistration is December 15th, 1988.

The **1990 International Systemic Congress** will be at Stirling, U.K. (Scotland) in late August or early September. *[Local organizer and Program Chair is Martin Davies, English Studies, University of Stirling, Stirling FK9 4LA, Scotland, UK].* The **1991 International Systemic Congress** will be in Tokyo, Japan. *[Local organizer: Fred Peng, Dept. of Linguistics, I.C.U., 10-2, 3 Chome, Osawa Mitaka, Tokyo, 181, Japan; Program Chair: Michael Halliday].*

Nominations for the **International Systemic Congress Committee** which will serve from 1989 through 1991 are requested by June 1, 1989. ■

ANNOUNCEMENTS

CONSISTENT CHANGE TABLE FOR TECHNICAL PAPERS. As a user of Manuscripter, do you wish the computer could align the interlinear glosses in linguistic examples while it is printing a grammar paper, so you would not have to align them yourself? Do you tire of typing complicated strings of Manuscripter dot commands each time you want to make a section heading or include a linguistic example? Would you like help in indexing your papers? Would you like to be able to set up special characters easily?

The answer has now been developed, in the form of a Consistent Change table called GENTXT.CCT. It lets you type your paper using backslash codes similar to those used in typing scripture portions and dictionaries. For example, \p starts a new Paragraph, \c starts a Centered line and \o starts indent carry-over or a hanging indent. To align a linguistic example with glosses under the vernacular words, all you need to type is \x. There is room for a number to the left of each example, and the change table can be set up to print diacritics and special characters in the same way as the scripture and dictionary change tables can. (Circumflex, acute and 'eng' are set up already, as examples for you to follow.)

The consistent change table, documentation and two related CC tables have been submitted as of September 1987 to International Computer Services for inclusion in the DEC, CP/M and MSDOS software libraries, so they can be ordered from there as the GENTXT package.

RENUMBERING EXAMPLES IN LINGUISTIC PAPERS. Now that the examples in linguistic papers are usually numbered sequentially, a problem arises when you want to add or delete some examples in the middle of the paper. You then have to go through and renumber all following examples in the paper. More than that, you also have to locate and renumber all the references to the examples, such as '. . . as in example 63a'.

But now help is available in the form of a program called RENUMX.PTP, which will do just that kind of renumbering. All it requires is that the example numbers follow the code \x and that references to those examples follow the code \xr. You tell RENUMX which examples you want to delete and where you want to insert new examples, and it does the rest. (The \x and \xr codes format the examples and references properly when printed with MS and the change table GENTXT.CCT.) RENUMX also has a mode in which it will check the examples in a number of files to make sure they are in correct numerical sequence.

The RENUMX program and documentation were also submitted to International Computer Services in September of 1987 for inclusion in the DEC, CP\M and MSDOS libraries, and can be ordered from there as the RENUMX package.

JIM HENDERSON Papua New Guinea ■

PAPERS CURRENTLY AVAILABLE FROM *WPFG*. *WPFG (Working Papers in Functional Grammar)* publishes papers which are *(a)* not (yet) ready for official publication, but sufficiently interesting as contributions to on-going discussions within FG and *(b)* papers that will be officially published, but whose publication will take at least one year after publication in WPFG. The following papers are available:

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Also available are Simon Dik's 1978 monograph *Stepwise lexical decomposition* (Hfl 5.00) and a complete bibliography of FG books, articles and papers (Hfl 2.50). These items can be ordered in the same way as a Working Paper. ■